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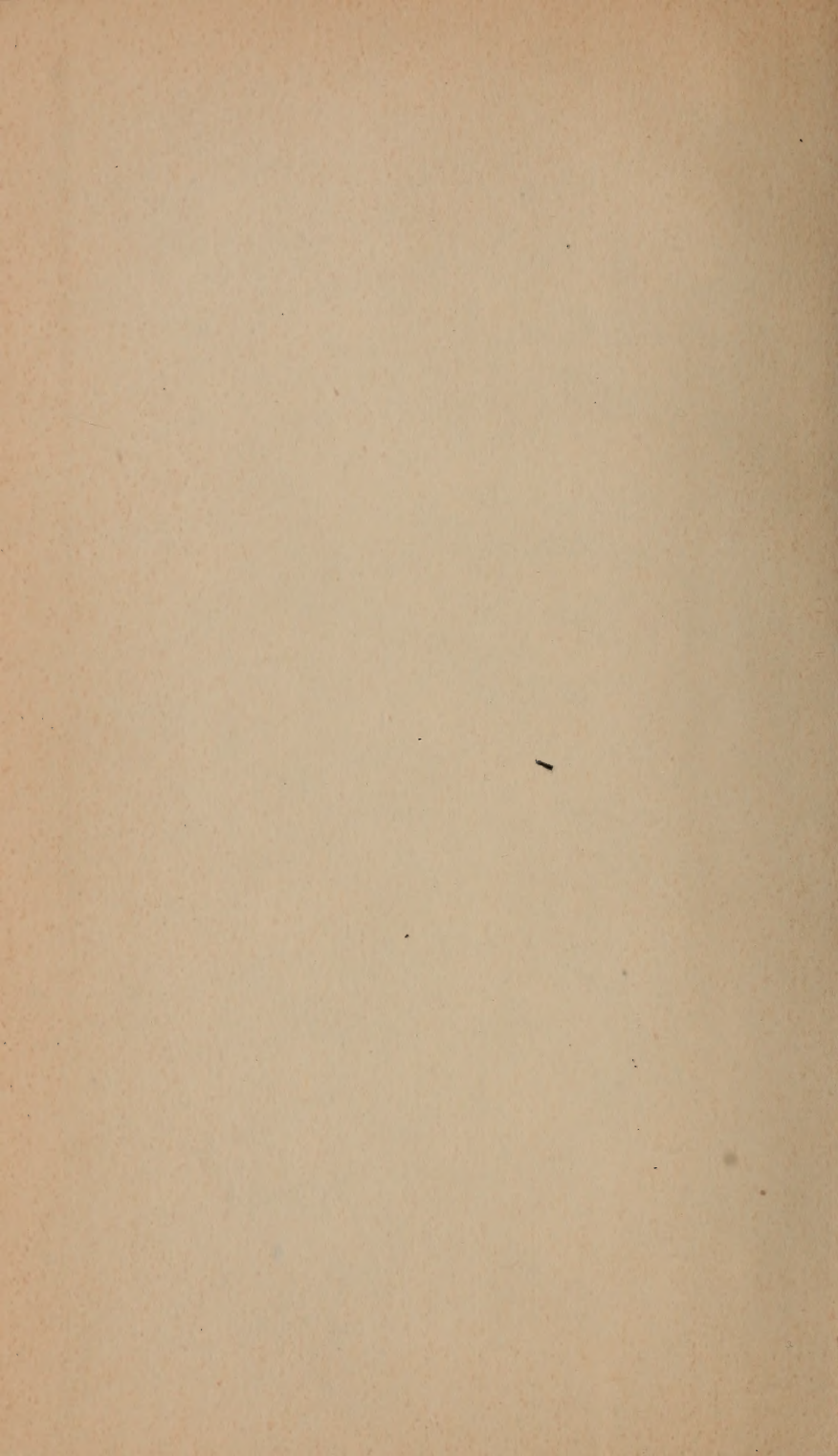
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CONDUCTED BY

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AND

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“Nec araneorum sane textus ideo melior quia ex se fila gignunt, nec noster
vilior quia ex alienis libamus ut apes.” JUST. LIPS. *Polit. lib. i. cap. 1. Not.*

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“Meditationis est perscrutari occulta; contemplationis est admirari
perspicua Admiratio generat quæstionem, quæstio investigationem,
investigatio inventionem.”—*Hugo de S. Victore.*

—“Cur spirent venti, cur terra dehiscat,
Cur mare turgescat, pelago cur tantus amaror,
Cur caput obscura Phœbus ferrugine condat,
Quid toties diros cogat flagrare cometas;
Quid pariat nubes, veniant cur fulmina cœlo,
Quo micet igne Iris, superos quis conciat orbes
Tam vario motu.”

J. B. Pinelli ad Mazonium.

32, 1 $\frac{1}{4}$.

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- IV. Illustrative of M. F. Kohlrausch's Papers:—on the Determination of the Absolute Value of the Siemens Mercury Unit of Electrical Resistance; and on a Variation-Barometer.

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[FOURTH SERIES.]

JANUARY 1874.

I. *On an Apparatus for Gas-analysis.*

By J. Y. BUCHANAN, *Chemist on board H.M.S. 'Challenger'**.

[With a Plate.]

A VERY important subject of investigation in the chemistry of the ocean is the nature and quantity of the atmospheric gases dissolved in it. These are extracted by boiling *in vacuo* in an apparatus recently described by Jacobsen†, and at the end of the operation are obtained hermetically sealed in a glass tube, in which they are capable of indefinite preservation. Although it would be absurd to waste time in making necessarily imperfect analyses of valuable specimens at sea, which are capable of being kept and analyzed with the greatest accuracy on shore, still cases might occur during a three years' voyage where it would be very desirable to make an approximate analysis of a specimen of gas, not necessarily proceeding from sea-water, but, for instance, from hot springs or volcanoes on shore.

The first desideratum for such an apparatus for use on ship-board appeared to be freedom from a mercurial trough. All existing apparatus, besides having this source of inconvenience, were far too large and cumbrous to be at all suited to the modest allowance of laboratory space available on board ship. What seemed to give most promise of success was in some way or other to adapt the original Ure's eudiometer to the purpose. The form of apparatus finally adopted is that represented in Plate I. fig. 1. It was constructed, according to my drawings

* Communicated by the Author.

† *Ann. der Chem. und Pharm.* vol. clxvii. p. 1.

and instructions, by Dr. Geissler, of Bonn; and it is needless to add that as a piece of glass workmanship it is a *chef-d'œuvre*.

Before describing it, it may be as well to state the objects sought to be attained, when the motives in the design of the various parts will be more apparent, and the measure in which they fulfil their end more correctly appreciated.

First of all, the size of the instrument must be reduced to a minimum; nor must its shape be so eccentric as to interfere with its being packed into a reasonable space of symmetrical form. Again, when packed it must be safe against rolling, and be easily unpacked and mounted, and as easily dismounted and packed away again. I wished further to be able to use eudiometrical as well as absorptiometrical methods, using in the latter liquid reagents. The necessity for a special gas-analysis room had to be dispensed with; and, as before mentioned, the use of a mercurial trough was to be avoided. The advantages secured by the peculiar way of packing, so that the case in which it is packed when out of use forms its support, and a working tray for saving spilled mercury when in use, were not contemplated from the beginning, but suggested themselves only after the glass work was finished, as until then the form and size of the case could not be determined on. It was made for me by Messrs. Kemp and Co., of Edinburgh; and the workmanship is in every way satisfactory.

The apparatus consists essentially of two U-tubes. The one, which according to precedent we may call the "laboratory tube," is wholly of glass; the other (the eudiometer) has the legs of glass united by an india-rubber tube of suitable length. These are affixed to mahogany boards A and B, which fit into the wings of the box C, where they are secured each by two bolts (z, z) passing into the sides of the box. D is a strong mahogany box for holding mercury.

The glass U-tube, which for convenience we shall call A, as well as the board to which it is attached, is 0.42 metre high; the shorter leg is 0.18 metre from the bend to where the capillary tube is joined on, and its diameter is 0.02 metre. The diameter of the capillary tubes is 0.003 metre. In the U-tube B the eudiometer (q) is 0.17 metre from capillary to india-rubber; the movable leg (p) is 0.34 metre long; and the diameter of both is the same as in A. The length of the box C is 0.45 metre, and the width and depth of each wing 0.195 metre and 0.105 metre respectively. The mercury-box D measures 0.12 metre by 0.10 metre by 0.08 metre, and when in use fits into the place where it is represented in the figure. When dismounted it is not packed in the case with the rest of the instrument.

Fig. 4.

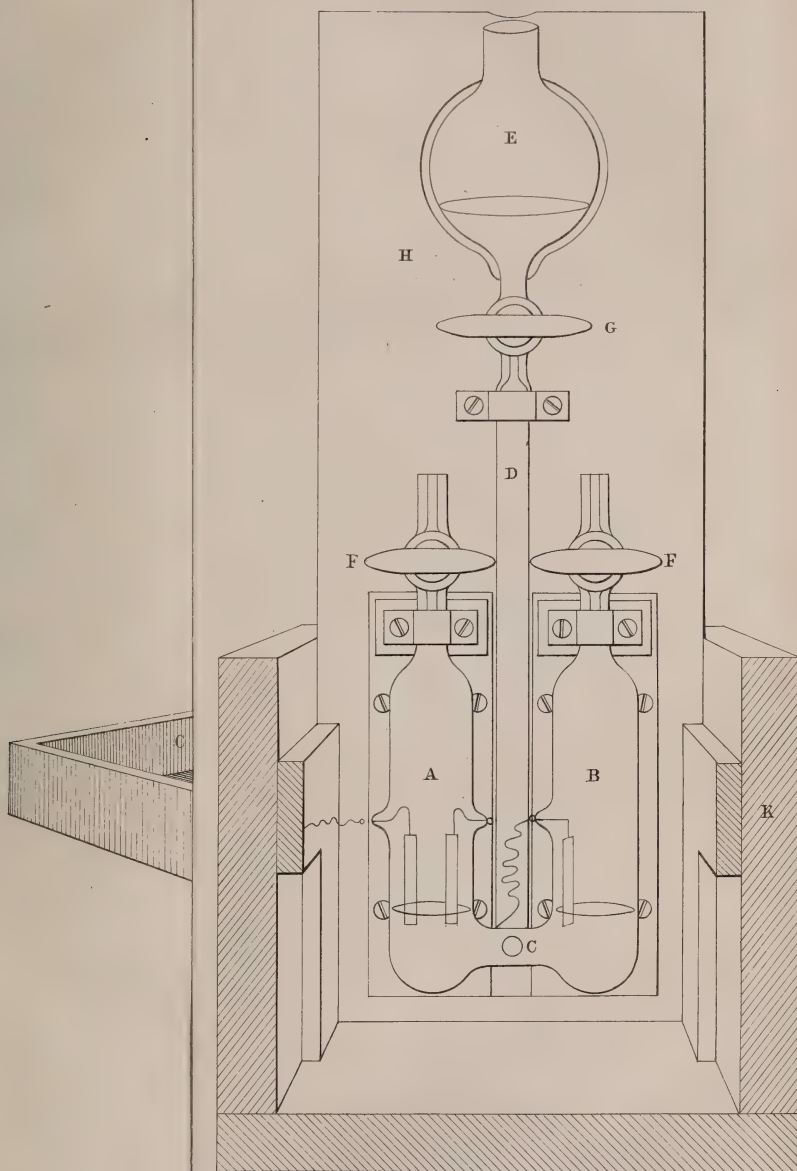


Fig. 1.

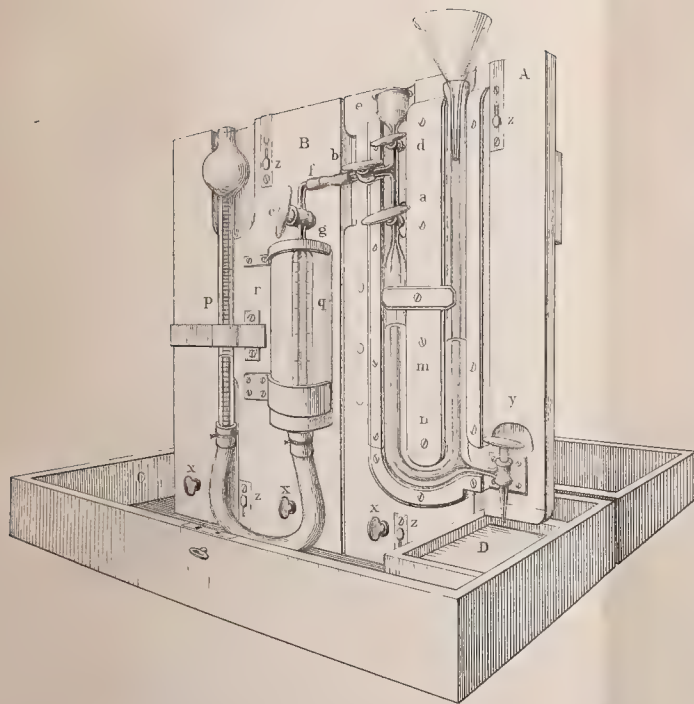


Fig. 2.

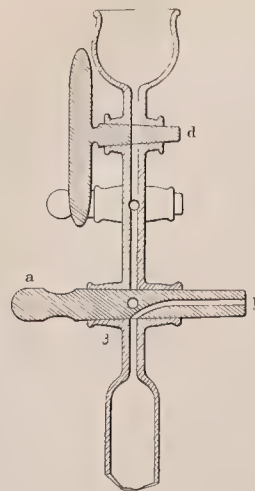


Fig. 3.

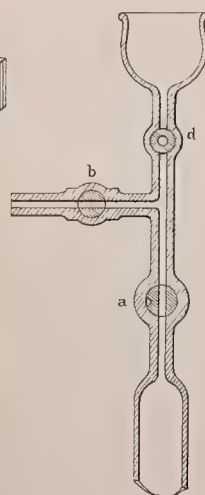
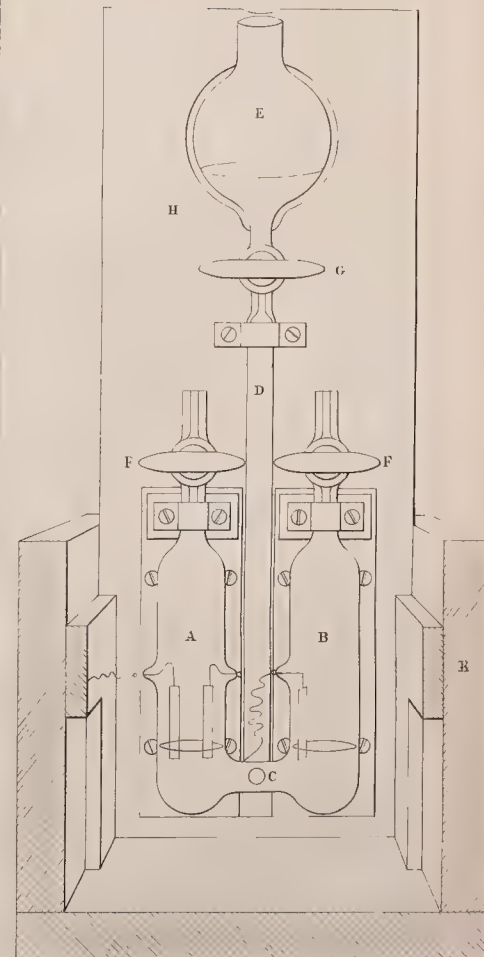


Fig. 4.



When in use the parts A and B are screwed to the back of one wing of the box, A with one and B with two screws (x, x, x). The back of the box carries four nuts, so that A may be fixed to it close up to the end of the box and with the stopcock y projecting over the side, leaving a space between A and B, the object of which will be explained further on.

The different parts of the apparatus will be best explained by describing the manipulations occurring during the analysis of a gas as well as those necessary for introducing the gas into the apparatus. But first we must describe the capillary part of the apparatus between the eudiometer g and the laboratory tube m . The part belonging to A is shown separately in two sections, figs. 2 and 3. The stopcock a has two tubes—the one affording direct communication upwards between the two portions of the capillary tube shown in fig. 3, the other communicating through the prolongation of the stopcock with the air, shown in fig. 2. b, c , and d are ordinary stopcocks pierced to the same bore as the tubes they connect. The capillaries of A and B are connected by a piece of india-rubber vacuum tubing. This tube is 0.003 metre diameter in the bore; and the thickness of its walls is likewise 3 millimetres. The stopcock d communicates with the air through the cup e . When the instrument has been set up as in fig. 1, it is filled with mercury by pouring in at the open legs of A and B, all the stopcocks being open, so that finally the mercury rises a little way up in the cup e . The apparatus being full of mercury, the gas to be analyzed is introduced into it either through the opening, γ , of the stopcock a , or, if it is contained in a sealed tube, by interposing it at f between the two parts A and B, A being for that purpose shifted to the further end of the box, and the two ends of the tube in which the gas is collected connected by vacuum-tubing, filled with mercury, with the ends of the capillaries of A and B. The pressure in one side of the apparatus is now reduced, either by running mercury from A by means of the stopcock y , or by lowering the movable leg p of B. When the points of the tube are broken the gas rushes into the parts of the apparatus where the pressure is least, its place being supplied by mercury from the other side. The stopcocks are then closed, the tube full of mercury removed, the parts A and B reunited by f , and the analysis proceeded with. When the gas is to be admitted through a , an india-rubber tube is connected at γ and filled with mercury by opening a , as in fig. 2, and running mercury through it. The point of the collecting-tube, having previously been touched with the file, is pushed into the india-rubber tube; the other end having also received a stroke of the file, is immersed in a cylinder full of mercury and the lower point broken off against the

bottom. The mercury immediately rises in the tube; and by supplying its place in the cylinder and reducing the pressure in A or B, as the case may be, the whole or part of the gas may be transferred to the instrument. Of course in all these cases the india-rubber tubes must be moistened inside with solution of corrosive sublimate.

The gas, having been introduced in one way or the other, is brought all into the eudiometer q , mercury being allowed to run over from A into q until the whole capillary part, down to where it joins the eudiometer at g , is full of mercury. q is a short eudiometer with a scale of only six divisions, like the measuring-tube in Frankland's apparatus*. It is immersed in the cylinder r , which is filled with water. The leg p is divided into millimetres, and is lowered or raised until the meniscus of the mercury stands exactly at one of the lines. The difference in height of the two columns is then measured by means of the millimetre-scale on p , and the temperature of the water in r ascertained. These, with the height of the barometer, give the volume of the gas. On the tube r marks are made (not shown in the drawing) corresponding to and on the same level with those on q . When the difference of level of the mercury in the two tubes is to be measured, p is applied to r and the level of the mercury in q read off, the marks on r giving the direction for the eye; that in p is then read off in the ordinary way, the difference giving the column of mercury to be added to or subtracted from the barometer-reading.

Let us suppose that we have a sample of air extracted from water. It consists of oxygen, nitrogen, and carbonic acid, the last of which is determined first by absorption with caustic potash. For this purpose mercury is run out of n by means of y , p is raised, and the stopcocks c , b , and a opened. The air is thus drawn over out of q into m , mercury being allowed to fill the capillary, when the cocks are again shut. The cup e is now filled with strong solution of caustic potash, and, the level in n being still kept low, the stopcock a is opened full, and then d very carefully, caustic potash being allowed to run down through a into m , where it meets the gas in the most advantageous way for quick absorption. When enough caustic potash has been allowed to enter, d is closed, some mercury poured into e , d again opened and the solution in the capillary replaced by mercury. When the absorption is finished, the level of the mercury in n is again raised and the stopcocks b and c opened. The stopcock a is now very carefully opened, the flow of the gas being further regu-

* The tube q in the figure is shown divided as stated in the text. I have found, however, that it is much more convenient to have it divided into millimetres; and the labour of calibration is no greater.

lated by raising or depressing p , and the gas allowed to pass over into q until the potash solution just touches the lower surface of the stopcock a , which, being open, has the position shown in fig. 3. The position of a is now shifted to that shown in fig. 2, when the potash solution is eliminated. The position of a is now brought back to that represented in fig. 3, and the gas remaining in the capillary swept out by mercury and the volume measured as above described.

If the oxygen is to be determined by absorption, the manipulations are exactly the same as in the case of carbonic acid, alkaline pyrogallie acid being used instead of caustic potash. If it is to be determined eudiometrically, then, after the carbonic acid has been absorbed, the gas remains in the eudiometer, the stopcocks c and b being shut. The stopcock d is now opened and a turned the reverse way to what it is in fig. 2—that is, with the side communication $\beta\gamma$ communicating with the capillary above a . The capillary is then emptied of mercury and the hydrogen-evolving apparatus connected with γ by means of an india-rubber tube, and the hydrogen allowed to stream through a , d , e until all air is swept out; the stopcock d is then closed and a brought back to its position in fig. 2, when the gas enters m . When enough hydrogen has passed in, a is brought to its position in fig. 3 and the hydrogen-apparatus dispensed with. Mercury is then poured into e , and d opened and the hydrogen in the capillary driven into m . The hydrogen is now passed over into the eudiometer and exploded, the measurements being made as described.

When the analysis is finished the mercury is emptied out of the tubes, the parts A and B bolted each into its own side of the box, the screws x , x , x being lodged in the nuts w , w , w let into the board A. The box when closed measures 0.48 metre by 0.22 metre by 0.22 metre, outside, and is easily portable. Of course the apparatus is not meant exclusively for work on board ship, where indeed attempts at gas-analysis should be avoided, as at the very best the results must be very uncertain; but it makes a compact laboratory apparatus and is economical in mercury.

For the preparation of the electrolytic gases, either separate or mixed, in a way convenient for gas-analysis, I have had the apparatus represented in fig. 4 constructed. It is of the well-known lecture-apparatus form, and consists of two tubes, A and B, united at their lower extremities by the short tube C, which connects them at the same time by means of the tube D with the reservoir E. At their upper extremities A and B terminate in capillary tubes furnished with stopcocks, F, F. Communication with the reservoir can be made or interrupted by the stop-

cock G. One of the tubes, A, is furnished with two platinum electrodes; the other, B, has but one. But delivery-tubes (not shown in the drawing) of the usual form fit upon the tubes above the stopcocks F, F. When about to be used, all the stopcocks are opened and diluted sulphuric acid poured in through the reservoir until it has eliminated all air and is running out itself at the delivery-tubes. The stopcock G is now shut and the battery connected, as circumstances may require, either with the two electrodes in A, or with one in A and one in B. Gas is allowed to escape freely until all dissolved air is eliminated. The stopcocks F F are then closed, G opened, and the liquid in A and B allowed to sink until it just covers the electrodes. G is then closed, and F F opened, when the gases may be introduced into the eudiometer in the ordinary way, or into the above-described apparatus in the way there indicated. Connexion between the gas-generator, the tubes A and B, and the reservoir E being shut off by the stopcock G, there is never any difficulty in forcing the gas through the mercury into the eudiometer.

The whole apparatus is attached to a mahogany board, H, which fits into the box K, shown cut through the middle in the drawing, either as represented, when in use, or, when not in use, as a lid, with the apparatus attached to its inner side. The box thus fulfils the double purpose of a convenient stand and a safe packing-case. The only alteration which I should be inclined to make, would be to have the lid to which the apparatus is attached made of vulcanite, to avoid the risk of the wood's warping in damp weather.

II. *On the Disintegration of the Electrodes in the Galvanic Arc of Light.* By HERMANN HERWIG*.

THE quantities of substance which are thrown off in fine powder from the electrodes of the voltaic arc have hitherto been determined only very uncertainly. To the statements hereupon, made chiefly by Van Breda† and Matteucci‡, a reduction to an otherwise well-known action of the current, perhaps to a simultaneous voltametric action of it, is wanting; and, besides, the experiments were made under circumstances which quantitatively can present only an indistinct picture of the disintegration, as will be seen from the following. Only a series of experiments by Grove§ on the present subject appears to wear a more precise

* Translated from a separate impression, communicated by the Author, from Poggendorff's *Annalen*, vol. cxlix. pp. 521-533.

† Pogg. *Ann.* vol. lxx. p. 326.

‡ *Comptes Rendus*, vol. xxx. p. 201; *Ann. de Chim. et de Phys.* S. 3. vol. xxxii. p. 350.

§ *Phil. Mag.* S. 3. vol. xvi. p. 478.

character ; and it is just this series which has led to extraordinarily surprising results. Grove placed in the arc of light a positive electrode of zinc over against a negative one of platinum, and in his experiments took into consideration only the disintegration which took place at the former. Now this zinc electrode was of such dimensions, and the intensity of the current so regulated, that the air should not occasion combustion of the massive electrode, but only of the fine particles thrown off from it. The air present was an isolated quantity ; and thus the amount of zinc thrown off could be calculated from the oxygen consumed, provided that all the disintegrated zinc, and this only, was oxidized. Grove believed that now every thing had been approximately fitted to the conditions of this experiment, and concluded from his results (which did not differ excessively from one another) that the quantity of zinc disintegrated was equivalent to that of the hydrogen occurring in a simultaneously inserted voltmeter.

The supposition herein expressed, of a surprising connexion between disintegration and electrolysis, has not, to my knowledge, been followed up by other investigators. Indeed the difficulty of satisfying, even in some measure, the conditions which Grove sought to realize in his experiment is extraordinarily great ; and this circumstance may very well have prevented the repetition of the experiments, as it seems adapted not to permit too much reliance on his data. It must further be said that the idea which Grove pursued in these experiments rested on erroneous assumptions. Namely, Grove thought that, if the electricity of the current could only effect its passage between the electrodes in combination with electrode-substance, then the quantity of electricity corresponding to a definite current-intensity would probably always determine the transport of a definite quantity of substance of the electrodes. But, leaving altogether out of consideration the decided improbability of the law of condition herein contained, the electrode-substance thrown off and that transported between the electrodes are quantitatively quite different. Van Breda's experiments have shown, and it will likewise be found in what follows, that only a portion of the disintegrated substance takes the path to the opposite electrode, while another portion flies about in all directions. Grove's experiments, however, are independent of this point of view ; for they would indeed express the quantity, not of the transported, but of the disintegrated zinc.

Independently, therefore, of the reason which induced him to make his experiment, it remains an open question whether there does not exist a simple connexion between the quantity of active electricity (that is, the intensity of the current) and the quantity

of particles subjected to the action of disintegration, perhaps as signified by Grove's experiment. Only it would be desirable to approach this question less indirectly and under the necessity of observing fewer conditions.

In the first place, it seems requisite to produce the luminous arc in a medium not chemically proportioned, between two chemically different electrodes, and by weighing and quantitative analysis of the electrodes to be clear about the amount of disintegration. The loss of weight of an electrode during an experiment, increased by the quantity of foreign metal taken up by it, would express the amount of substance which it has thrown off. I took some pains to make experiments of this kind in hydrogen gas; but, with the battery I employed (50 Grove's elements of the usual size), only a very imperfect arc of light was obtained in hydrogen, and I did not arrive at any very exact results.

I therefore replaced the hydrogen surrounding of the electrodes by as nearly perfect a vacuum as possible, in which the arc is obtained with decidedly greater facility. The electrodes, screwed upon strong iron rods, were led through two large caoutchouc stoppers, which tightly closed the two ends of a strong cylinder such as is used for lamps. A glass tube with a glass cock was also passed through one of the corks, and, for the purpose of exhausting the cylinder, was connected with a Geissler air-pump. The air in the cylinder was thus reduced to 1 or 2 millims. pressure. The electrodes were placed at an average distance of 1 millim. At the commencement I introduced the luminous arc, as proposed by Herschel*, by means of a Leyden jar. Meanwhile it appeared that the operation could be as well performed by taking advantage of the elasticity of the caoutchouc and bringing the electrodes together by a momentary pressure with the hands. They spring asunder again immediately, before a perceptible quantity of hydrogen is developed in the voltameter through which the current simultaneously passes; and thus the bright arc is instantly formed with the fine metallic dust flying in all directions towards the sides of the cylinder. This spirting of metallic dust-particles (which, as said, directly commences) is characteristic for this, that the arc constitutes the only intermedium between the in every other respect separated electrodes, and therefore proceeds in the manner desired.

There is a series of metals which show themselves the least convenient for these experiments; they are the easily fusible ones, from which, with a somewhat continued action of the current, large portions readily melt off and so render difficult the determination of the quantities really separated in dust. It thus happened in experiments with zinc, cadmium, lead, and tin.

* Pogg. *Ann.* vol. xlix. p. 122.

On this account I confined myself at first to the investigation of iron, nickel, and copper.

In the experiments with these metals the results showed not the remotest approach to regularity in respect of the quantities disintegrated. I content myself with bringing forward a few experiments and remarking only that the electrodes formed cylinders rounded at the fore end, of at most 4 or 5 grammes weight.

Experiment 1.

Hydrogen developed in the voltameter, 31 cub. centims. or 2·78 milligr.

The positive electrode of iron, lost 7 milligr. iron, and took up 3 milligr. copper.

The negative electrode of copper, lost 4 milligr. copper, took up 1 milligr. iron.

Experiment 2.

Hydrogen developed in the voltameter, 20 cub. centims. = 1·78 milligr.

Positive electrode iron, lost 5 milligr. iron, took up 4 milligr. copper.

Negative electrode copper, lost 5 milligr. copper, took up 0·5 milligr. iron.

Experiment 3.

Hydrogen developed in the voltameter, 65 cub. centims. = 5·82 milligr.

Positive electrode iron, lost 36 milligr. iron, took up no copper.

Negative electrode copper, lost 10 milligr. copper, took up 3 milligr. iron.

Experiment 4.

Hydrogen developed in the voltameter, 48 cub. centims. = 4·3 milligr.

Positive electrode iron, lost 6 milligr. iron, took up no copper.

Negative electrode copper, lost 9 milligr. copper, took up 1 milligr. iron.

Experiment 5.

Hydrogen developed in the voltameter, 89 cub. centims. = 7·97 milligr.

Positive electrode iron, lost 9 milligr. iron, took up a trace of copper.

Negative electrode copper, lost 18 milligr. copper, took up 7 milligr. iron.

Experiment 6.

Hydrogen developed in the voltameter, 60 cub. centims. = 5·38 milligr.

Positive electrode copper, lost 8 milligr. copper, took up 4·5 milligr. iron.

Negative electrode iron, lost 5.5 milligr. iron, took up 1 milligr. copper.

The longer of these experiments had to be performed with short intermissions, in order not to heat the electrodes to fusion. As very small quantities were in question, the chemical analysis was of course cautiously effected, and was only applied to the superficial layers of the electrodes, these being the only ones concerned. Since, in spite of the extreme rarefaction of the air in the cylinder, a few small portions on the electrodes (especially the particles of other metals precipitated upon them) were sometimes found to be oxidized, this slight content of oxygen had to be taken into account in the analyses. Although under these circumstances the numbers given present little uncertainties, yet they cannot be affected with gross errors; and therefore we have in them a satisfactorily approximate representation of the proportions.

Therefrom these proportions are perceived to be absolutely irregular. Even when the same metals are employed respectively for the positive and for the negative electrodes in different experiments, they give sometimes equal loss at both electrodes, sometimes more at the positive, and sometimes more at the negative electrode. There is likewise visible no connexion with the amount of hydrogen simultaneously developed. The only regularly occurring circumstance in these, as in all my experiments, was, that the heating of the positive electrode was always the quickest, and it was the first to become red-hot; but this circumstance, in the case of two electrodes of the same kind, is universally known.

Now, it seems, a cause can at once be adduced which contains a partial explanation of this irregularity. The experiments above given show that, although a brisk scattering of the disintegrated metallic particles in all directions always takes place, and in consequence of this a more or less dense precipitate of powder is always seen on the neighbouring sides of the cylinder, yet a not inconsiderable portion of the metal lost by one electrode may often be found on the other. Such portions of the foreign metal are firmly melted to the electrode on which they are seated. Now, supposing an experiment in which this has taken place continued, doubtless one of those portions might again be subjected to disintegration, since it is situated in the front, the part most affected by it; and thus the act of disintegration would have been employed more than once upon the same metallic particles. That such a behaviour actually played a part in the experiments here discussed is shown by several observations. For example, in experiment 1 the 3 milligr. of copper taken up by the iron electrode was not on its surface, but under a thin layer of iron,

which had evidently, therefore, come later, doubtless thrown back again from the copper electrode, as the iron electrode itself was certainly not heated to fusion. It is thus seen that an electrode, under the circumstances present in these experiments, may take up again already-disintegrated metal and subject it to a fresh disintegration. Consequently the numerical results of all such experiments are completely illusory.

More success could perhaps be expected from an arrangement in which chemical alterations of the disintegrated metal are taken advantage of to avoid repeated disintegrations; and this comes nearer to Grove's experiment. If in experiments in air the powdered metal were completely oxidized and thus presented no mechanically conducting connexion with the rest of the electrode, this purpose would perhaps have been attained. But determinations according to this method have not only the difficulty experienced by Grove, namely that oxidation readily takes place also on the mass of the electrode, but also one of an exactly opposite nature; and even this makes them as ill adapted as the previous determinations to answer the question proposed. That is to say, in general the whole of the disintegrated substance of an oxidable electrode is not actually oxidated in air, but the oxidation extends more towards the circumference of the arc of light, while pure metal readily passes in its interior.

On this I have made various experiments, and will only cite one case, which speaks distinctly. I placed two electrodes of nickel opposite each other in air, and caused the arc to pass over, during which 60 cub. centims. of hydrogen were developed in the voltameter. Both electrodes obtained an oxidized layer, which with a little care could be very well separated clean from the metal. After this the positive electrode gave a loss of weight of 4 milligr., and the negative one of 13 milligr. But the positive electrode showed in front a projecting piece of pure metal parted off from the rest of the electrode by a fine fissure. I sprung it off at the fissure, and found that it had been seated on a fully oxidated base. This piece had therefore first arrived thither during the experiment, and, indeed (since neither electrode had once become red-hot), from the opposite electrode. It was, however, oxidated only on the outermost surface; throughout its interior was pure metal: it amounted to 6 milligr.

It is thus just as little possible to measure in this way. Also an artificial blast of air or oxygen into the arc would hardly give serviceable experiments.

It may therefore certainly be maintained generally that no experiments can answer our question in which both electrodes take part in the disintegration. We must accordingly endeavour to take away the cooperation of one of the electrodes. For this

I have tried the following means, and ascertained its perfect utility.

For one electrode I took a large, thick plate of copper; for the other, in order to have a metal not oxidable in air, a knob of pure silver. The latter was screwed on an iron rod, which, wrapped round with a spiral spring, was passed through a thick caoutchouc cork, which bore three insulated supports running from the sides of the rod. If the copper plate was touched with the feet of these, the silver knob could, by a screw-arrangement on the iron rod, be brought as near to the plate as was desired. It was then only necessary to press the head of the iron rod (which at the same time contained the polar wire), in order by a very brief contact between the silver knob and the plate to introduce the luminous arc. The spiral spring immediately jerked the silver knob back to its previous position.

The arc thus produced, which was maintained very perfect with a battery of 36 Grove's elements, I now carried over continually new points, by holding the caoutchouc cork in my hand and making the three insulated supports glide over the plate. In this manner the end of the arc was always formed by a fresh point; so that, on the part of the silver that had once gone over, repeated action was not possible. With respect to the copper itself, it was to be expected that nowhere would any material disintegration take place; for the heating was distributed more uniformly over the considerable mass of the entire plate, which never became so hot that the hand could not have been held for a long time in contact with it. Indeed the result was this—that never was the slightest trace of copper found on the silver knob, and scarcely anywhere on the plate was a layer of cupreous oxide to be perceived, which otherwise very readily forms on heated copper. The copper, then, remained substantially intact; and thus the conditions prescribed for the experiment were fulfilled.

The one-sided disintegration of the silver electrode in these experiments manifested itself in a form a little complicated. First, following the course of the arc, a stripe of bright metallic silver was sharply impressed into the copper; but this was succeeded by a rather thin layer above and on each side of it, and more or less impressed, of black powder, which, cautiously removed from the plate and analyzed, proved to be a silver-compound, probably an oxide. This powder, it seems, forms at the periphery of the arc, and consequently where the air has access. It is therefore deposited on the plate sideways of the proper course of the arc, and of course, moreover, where at each moment the arc itself has preceded it, consequently above the stripe of metallic silver. Where, however, the arc is interrupted by sud-

denly moving the silver knob away from the plate, it does not make its appearance; there metallic silver alone is to be seen. Precisely the same powder is seated, at the commencement of an experiment, on the knob itself; but it vanishes there, and does not appear again, as soon as the knob approaches a red heat, by which it is reduced to the metallic condition. It is not an improbable conjecture that this oxidation of the silver is to be ascribed to the presence of ozone in the voltaic arc produced in air; and therein might be seen a further argument for the opinion entertained probably by the majority of physicists—that in the voltaic arc the movement of the electricity is not brought about exclusively by the transport of the substance of the electrodes, but that a partial leap of the intervening layers of gas takes place, analogous to the induction-spark†.

Now the quantity of the silver that goes over in this way (that is, the disintegrated powder) can be settled by simply weighing it, since no permanent oxidation of the knob itself takes place. I obtained thus, with two silver knobs of from 5 to 6 grammes weight, the following results in the order given:—

Silver Knob I.

The knob was	Hydrogen developed in the voltameter, in		Loss of silver, in milligr.	Ratio of the weight of the silver lost to the hydrogen developed.
	cub. centims.	milligr.		
+ electrode	22	1·97	6·2	3·15
+ "	32·5	2·91	16	5·50
+ "	24·6	2·20	14	6·36
+ "	85·6	7·67	51·4	6·70
— "	76·4	6·84	9	1·32
+ "	85	7·60	34	4·47*
+ "	29·8	2·67	16	6
— "	28·8	2·58	12	4·65*
— "	98	8·78	38·5	4·39

Silver Knob II.

+ "	29	2·60	6	2·31
— "	53·5	4·79	7	1·47
+ "	168·8	15·12	113	7·47
+ "	29·4	2·63	16	6·09
+ "	76·7	6·87	51	7·43
— "	70·7	6·33	26	4·09
+ "	168·2	15·07	130·5	8·66
+ "	44·8	4·01	21	5·24*

† Compare also Grove's account (Phil. Mag. S. 4. vol. iv. p. 498) of similar phenomena of oxidation of silver in the induction-spark.

The numbers in the last column show very clearly :—

(1) That the loss of silver is never even distantly equivalent to the amount of hydrogen developed ;

(2) That the knobs suffer greater losses, under otherwise like conditions, the oftener they have already served for experiments—that is, the more their form has been already broken up and become adapted to the passing arc of light ;

(3) That in experiments of longer duration (where of course a brief interruption was always made when the knobs began to approach the temperature of fusion) the losses are more considerable ; that is, higher temperature of the knobs determines greater losses ;

(4) That the positive electrode of silver suffers smaller losses when the opposite parts of the negative plate are dissimilar in constitution ; for the experiments marked with an asterisk were made with a copper plate which had been first prepared at the turning-lathe, and presented concentric higher and deeper portions. In the commencing experiments the silver knobs were frequently carried transversely over these unevennesses.

(5) That the knobs taken for the positive electrode give greater losses.

It does not, then, depend exclusively on the chemical nature of the electrodes and the intensity of the current, but much more on manifold circumstances, how great the quantity of the disintegrated material shall be.

For these experiments, if they are to be performed in the preceding simple form, only non-oxidable electrodes can with advantage be used. I have not on this account deemed it necessary to follow up the experiments further with expensive metals of this sort, because those communicated are quite sufficient to definitively cut off all supposition of any simple connexion between the intensity of the current and the disintegration of the electrodes.

Aachen, April 4, 1873.

III. *A Theory of the Source of Terrestrial Magnetism.*

By Professor CHALLIS, M.A., F.R.S., F.R.A.S.*

IT may facilitate the understanding of the theory which is the subject of this communication if I begin with indicating the writings in which I have already discussed the theory of magnetism generally, and make some remarks on the views included therein as to the specific character of the earth's magnetism.

* Communicated by the Author.

My first researches relative to a theory of magnetic force were published in a communication consisting of Parts I. and II. contained in the Numbers of the Philosophical Magazine for January and February 1861. The account of the source of terrestrial magnetism given in articles 27–30 of that communication agrees for the most part with the views I hold at the present time, especially as regards the explanation in art. 27 of the generation of magnetic streams of the æther by the impulses it receives from the component atoms of the earth in their rotation about the earth's axis. Also I consider it to be correctly asserted in art. 33 that no recognizable magnetic current results either from the motion of the earth in its orbit, or from that due to the motion of translation of the solar system in space. This assertion, however, is partly dependent on facts of observation, no independent theoretical reason being there given for it.

Some discussion of the essential principles of the hydrodynamical theories of magnetic force, and of other physical forces, will be found in the Philosophical Magazine for April 1861 (p. 250).

Two papers in the Numbers of the Philosophical Magazine for February and March 1863 treat (1) of the zodiacal light; (2) of the analogous phenomenon of a uniform luminous arch seen at night across the sky, and symmetrically disposed about the ecliptic; (3) of diffused nocturnal luminosity. These phenomena are considered to give ocular evidence of the existence of solar and terrestrial magnetic streams.

In my work '*On the Principles of Mathematics and Physics*' (published in 1869), the Theory of Magnetic Force occupies pages 604–676, including, in pages 613–618, a theory of terrestrial magnetism. In discussing the source of the earth's magnetism I have there maintained, in contradiction to an unsustained assertion in art. 28 of the before-cited communication, that the earth by its motion does not carry with it any sensible amount of the æther. It is, however, to be said that no distinct account is given in that work of the respective effects of the orbital and rotatory motions of the earth in generating magnetic streams.

With the view of making the hydrodynamical theory of magnetism more complete, I subsequently went through a new discussion of its mathematical principles, which is published in the Philosophical Magazine for June 1872. Lastly, in a work entitled "*An Essay on the Mathematical Principles of Physics*," produced in the present year, I have brought under review the whole of the hydrodynamical theory of magnetism, and in such manner that as little use as possible is made of analytical symbols.

In arts. 120-122 and 130-132 of this publication, *theoretical* reasons are given for concluding that little or no magnetic effect is attributable to the earth's motion of translation, and that terrestrial magnetism is almost wholly due to streams generated by the rotation about its axis. For the sake of convenience, I propose to reproduce here the reasoning by which these conclusions are there arrived at, previously to adding other considerations confirmatory of them. As the writings above enumerated sufficiently indicate the way in which the hydrodynamical theory of magnetism accounts generally for magnetic force by the action of steady streams of the æther, it will only be necessary for the present purpose to investigate the mode of generation of those steady streams by the agency of which the principal facts of the proper magnetism of the earth may admit of explanation.

It must first be mentioned that the philosophical principles I have so long advocated forbid making any other hypotheses respecting atoms and the æther than those I have frequently had occasion to enunciate in this Journal. Accordingly it will be supposed that the earth consists wholly of inert spherical atoms of constant form and magnitude—that they are held in positions of equilibrium by atomic and molecular forces, which are only modes of action of the æther itself, which is assumed to be the source of all active force, and to press always and everywhere proportionally to its density—and that consequently the æther at rest has the same density within the earth as in the space outside. Moreover it is supposed that the occupation of space by the finite atoms is very small compared with the intervening spaces, even at the earth's centre, where the atomic density is greatest.

Considering, first, the motions which the atoms impress on the æther by reason of the earth's *orbital* motion, we may at once infer from Poisson's solution of the problem of the simultaneous movements of a ball-pendulum and the surrounding air, the motions of the æther which will be produced by any single atom; and since the motions due to different atoms, as being *steady* relatively to the positions of their centres, may coexist, we may conclude, on the hypothesis that the proportion of space occupied by the atoms is very small, that the total motion is *quam proxime* the sum of all the motions caused by individual atoms. Also since, according to that solution, just as much fluid flows backwards as the atom displaces by its forward motion, it follows that during the time the earth is moving over a portion of its orbit equal to its diameter, a quantity of fluid crosses the transverse plane which is tangential to its surface in the first and last positions, equal to the quantity which would just fill the

space occupied by the atoms. Hence, if we supposed this quantity to flow in that interval in a uniform concentrated stream through a circular aperture of radius equal to that of the earth, it might readily be shown that the ratio of its velocity to the velocity of the earth is two thirds of the ratio of the space occupied by the atoms to the earth's volume. This ratio, by hypothesis, is extremely small; and therefore it would not be contradictory to the theory to find that but little magnetic effect is due to ætherial currents at the earth's surface produced by the orbital motion.

The motions resulting from the composition of all those severally impressed by the atoms will evidently be symmetrical with respect to an axis passing through the earth's centre in the direction of the motion, and fulfil at the same time the condition that just as much fluid enters at the following hemispherical surface as issues at the preceding one. The total motion relative to the position of the earth's centre at any time will consequently be a *circulating* motion; and a plane through the centre transverse to the direction of the earth's motion will be crossed by the *refluent* streams at right angles. The disturbances of a magnet's declination by these currents would have two maxima and two minima in the course of a revolution of the earth about its axis. Like effect would result from the earth's motion of translation due to the motion of the solar system in space.

But no variations of magnetic declination subject to a regular law of this kind have been detected by observation, excepting one of very small amount traceable to the influence of the moon. It follows, therefore, that experience confirms the foregoing *theoretical* inference, that the currents generated by the earth's motions of translation have little or no magnetic effect.

It will appear from a consideration of the foregoing argument that the earth, conceived to consist of discrete atoms, is incapable, by reason of the resistance arising from the inertia of the unlimited mass of the æther, to impress either in its interior or its immediate neighbourhood, motions whereby any portion of the æther could be permanently transported across a given fixed plane. The only possible motions are those necessary for vacating and filling up the spaces successively occupied by the atoms in their onward course, which motions, as we have seen, are not recognizable by any magnetic effects, and may therefore be supposed to be extremely small compared with the earth's velocity. The case is otherwise with respect to the streams due to the earth's rotation, the generation of which may be conceived of as follows.

As the resultants of the movements immediately impressed on
Phil. Mag. S. 4. Vol. 47. No. 309. Jan. 1874. C

the æther by the revolution of the atoms about the earth's axis are circular, there will be no such reflux streams due to the inertia of the general mass of the æther as those produced by the earth's motion in its orbit. In fact, so long as there is any relative circular motion between the æther and the atoms, the latter will continually impel the æther, till the relative motion is reduced to zero. It may therefore be inferred that in the ultimate or steady state of the motion, the *circular* motion of the æther within the earth is the same as that of the atoms by which it is immediately impressed. This circular motion will, by reason of the centrifugal force, operate upon the æther external to the earth; and the condition of steadiness requires that the external æther should partake of a certain amount of circular motion decreasing with the distance from the axis of revolution, in order that the whole tendency of the centrifugal force transversely to the axis may be counteracted by the inertia of the surrounding mass of the æther. In short, if we conceive to be impressed on the earth and the æther a motion equal and contrary to the earth's orbital motion, the result of combining this motion of progression with the circular motion would be a composite *spiral* motion of the æther having always at the same points the same relations to the fixed centre of the earth. Hence, the earth being now supposed to move, this spiral form of steady motion would be capable of travelling uniformly through space in company with the earth. (See a discussion of this kind of motion in the article on the theory of magnetism in the *Philosophical Magazine* for June 1872.)

But it is evident that this rotatory motion will tend to draw the æther from the axis equally in all directions in any given transverse plane, and thus to cause the external æther to rush in at the polar parts to supply any partial vacancy which the centrifugal force might produce. If, however, the circumstances of the earth were in all respects symmetrical with respect to the equatorial plane, these tendencies would just counteract each other, and no currents, other than the circular movements, would be generated; for the circulating currents which the influx at one pole would produce of itself would be just neutralized by those due to the influx at the other, and in consequence the influx at each would be stopped. But because the circumstances of the solid and fluid parts of the earth are not symmetrical with respect to the equatorial plane, there will be a differential action due to the centrifugal force, whereby streams will be produced which, as satisfying the necessary condition of *circulating*, will be permanent relatively to the earth. The direction of the flow will be determined by the circumstances which violate the condition of symmetry; and it is therefore reasonable to suppose,

since there is a preponderance of land in the northern hemisphere, and the currents are always towards the positions of greatest atom-density, that they will flow out of the northern parts and, after circulating, enter into the southern parts. This is uniformly taken to be the direction of the earth's currents in my hydrodynamical theory of magnetism.

Accordingly the positions of greatest efflux and influx, although they cannot be far distant from the earth's poles, will be partly dependent on the distribution of land and water. It is thus consistent with the theory to find that in the northern hemisphere there are *two* positions of maximum intensity—one, which at present appears to be the principal one, in a high latitude of the North-American continent, and the other a little north of the Asiatic continent. There is not the same geographical reason for two positions of maximum influx in the southern hemisphere; and, in fact, observation has hitherto only determined that, if there be two, they cannot be far apart. Hence the theory at once accounts for the circumstance that, of all positions on the earth's surface, the magnetic intensity is greatest in the neighbourhood of the south pole. For the total influx is necessarily equal to the total efflux; and consequently the influx is intensified in that quarter, either because there is but one position of maximum intensity, or, if there be two, because they are near each other. The foregoing comparisons of the theory with facts of observation are confirmatory of the view taken of the nature and source of terrestrial magnetism.

It will be seen that this theory attributes the generation of terrestrial magnetic streams altogether to impulses given to the æther by the earth's atoms in motion, and that it would be much corroborated by any independent evidence of the actuality of this action between atoms and the æther. Now it happens that such evidence is obtainable by reference to the explanation of certain phenomena of *aberration*, which I have given in the Number of the Philosophical Magazine for April 1872, inasmuch as that explanation depends on an impulsive atomic action of the very same kind as that which has been under consideration, as will appear from the following argument.

The aberration of light is simply due to the circumstance that whereas the pointing of a telescope, as *instrumentally* determined, is in the direction of the straight line joining the optical centre of the object-glass, or object-mirror, and a certain fixed point in the field of view, the actual course of a ray from the first point to the other deviates from that line by reason of the earth's movement in the interval occupied by the passage of the ray. The deviation is in the direction towards which the earth is moving; and its angular amount is measured by the ratio of the earth's

velocity, resolved perpendicularly to the line of pointing, to the velocity of light. Now the communication above cited contains an account of experiments made with telescopes the tubes of which were partly filled with a transparent fluid, the object of this arrangement being to ascertain what effect would thereby be produced on the amount of aberration. I confess that I fully expected, on account of the longer time occupied by the passage of the light between the two points owing to the retardation caused by the fluid column, that the amount of aberration would by this means be *increased*. But two independent sets of experiments, one by M. Hoek with the telescope pointed to a terrestrial object, and the other by the Astronomer Royal with the telescope directed to γ Draconis, have proved conclusively that the passage of the light through the fluid column does not sensibly alter the amount of aberration. By parity of reasoning the aberration is unaffected by transmission of the rays through the lenses of the telescope. At the end of the aforesaid communication I have endeavoured to account for these facts theoretically, but not quite successfully. The following amended explanation may, I think, be considered to be satisfactory.

The rate of propagation of any disturbance of the æther being supposed to be a in free æther, that within a medium whose refractive index is μ is $\frac{a}{\mu}$. Hence the mean retardation of the rate of propagation by the reaction of the atoms is $a\left(1 - \frac{1}{\mu}\right)$, which, if a^2 represent the elasticity of the æther, may be supposed to be due to a retarding force represented by $a^2\left(1 - \frac{1}{\mu}\right)^2$. Conceive the propagation to take place along the line of pointing of a telescope in the tube of which there is a fluid column of refractive index μ , and let the telescope be carried by the earth's motion with a velocity the projection of which transverse to the line of pointing is V . Hence the æther will be continually impelled by the atoms of the fluid in the transverse direction proportionally to that resolved part of the earth's velocity. Like impulses will be produced by the part resolved along the line of pointing; but as these, as far as regards aberration, would only give rise to small quantities of the second order, they may be left out of account.

It has been already argued above that the disturbance of the æther produced by the motion of translation of the earth is steady motion of such kind that it is always *quam proxime* the same at the same position relative to the earth's centre. Hence the disturbance is propagated in space at the rate of the earth's

motion; and any mutual action between the æther and the atoms is propagated at the same rate. Consequently the rate of propagation resolved transversely to the line of pointing is the velocity V . Now since, as we have seen, when the independent rate of propagation is a , the mutual action between the æther and the atoms of the fluid column is represented by $a^2 \left(1 - \frac{1}{\mu}\right)^2$, it follows, analogously, that when the independent rate of propagation is V , the mutual action between the æther and the atoms of the same fluid is $V^2 \left(1 - \frac{1}{\mu}\right)^2$. This amount of *apparent* elasticity corresponds to a rate of propagation equal to $V \left(1 - \frac{1}{\mu}\right)$ in the transverse direction. Combining this with the rate of propagation $\frac{a}{\mu}$ in the line of pointing, the result is an aberration from that line the angular measure of which is the ratio of $V \left(1 - \frac{1}{\mu}\right)$ to $\frac{a}{\mu}$, or $\frac{V}{a}(\mu - 1)$. By comparing the direction of the earth's motion with the direction of the propagation of light towards the earth, it will be seen that *this* aberration is *from* the quarter towards which the earth is moving, the ray being, as it were, *dragged* towards that quarter while it traverses the part of its course from the optical centre of the telescope to the field of view, which lies within the fluid column. Relatively, therefore, to the usual mode of estimating aberration, this amount is *negative*.

The *instrumental* aberration, as is known, is $\frac{V}{a}$, apart from the effect of the passage of the light through any medium in the telescope-tube. For a medium of refractive index μ this ratio is altered to $\frac{V\mu}{a}$, on account of the rate of propagation in the medium being to that in vacuum in the ratio of 1 to μ . As the sign of this aberration is *positive*, the total aberration is $\frac{V\mu}{a} - \frac{V}{a}(\mu - 1)$, or $\frac{V}{a}$, which is independent of μ . Since the same reasoning applies to any portion of a given medium, and therefore to the whole of each medium, it follows, conformably with experience, that the amount of aberration is unaffected by the transmission of the light through any number of transparent media contained within the telescope-tube.

I cannot forbear remarking in conclusion, since the same mutual action between the atoms of a substance and the æther has

been adduced to explain two such different phenomena as the amount of aberration under the circumstances above detailed, and the existence of terrestrial magnetic currents, that I am justified in regarding this coincidence as corroborative of the truth of both explanations.

Cambridge, November 17, 1873.

IV. On Wheatstone's Bridge. By R. S. BROUGH, Assistant Superintendent of Government Telegraphs*.

IN a paper by Mr. Heaviside, published in the Philosophical Magazine of February 1873, "On the best Arrangement of Wheatstone's Bridge for measuring a given resistance," two equations are given (the author's Nos. 1 and 2) to express generally the strength of the current flowing through the galvanometer before balance is established. These equations are incorrect, and they should respectively stand as follows:—

$$u = \frac{(ad - bc)E}{(a+c)(b+d)e + (a+b+c+d)ef + (a+b)(c+d)f + cd(a+b) + ab(c+d)}$$

and

$$u = \frac{\frac{ad - bc}{a + b + c + d} E}{\left\{ \frac{(a+b)(c+d)}{a+b+c+d} + e \right\} \left\{ \frac{(a+c)(b+d)}{a+b+c+d} + f \right\} + \left\{ \frac{ad - bc}{a + b + c + d} \right\}^2}.$$

From the latter equation the best resistance for the galvanometer *near balance* can be at once deduced; for near balance the term $\left\{ \frac{ad - bc}{a + b + c + d} \right\}^2$ may be neglected against the remainder of the denominator of the right-hand member of the equation, and then we have for a maximum of $u\sqrt{e}$:—

$$E = \frac{(a+b)(c+d)}{a+b+c+d}.$$

Obviously the question of the best resistance of the galvanometer is only independent of the resistance of the battery (so long at least as this is a quantity of relatively finite magnitude) when balance is nearly established, and is not so generally (*i. e.* away from balance as well as at balance) as Mr. Heaviside's equations would imply.

It is to be observed, however, that to proceed directly to find the resistance of the galvanometer which makes its magnetic effect a maximum *at balance* (when no current passes through

* Communicated by the Author.

the galvanometer) is a merely mathematical problem destitute of physical meaning. Clearly the proper procedure is first to prove that there exists a general maximum when balance is not established, dependent on the resistances of the four branches and on the resistance of the battery, and then to find to what condition this converges as we approach balance and becomes equal when we reach balance.

From the second of the above equations we find that the general maximum is given by

$$E = \frac{(a+b)(c+d)}{a+b+c+d} + \frac{(ad-bc)^2}{(a+b+c+d)\{(a+c)(b+d) + (a+b+c+d)f\}},$$

which, as balance is approached, rapidly converges towards the condition

$$E = \frac{(a+b)(c+d)}{a+b+c+d}.$$

This is the general useful solution, since it is only near balance that it is of practical importance that the condition of maximum sensibility should be fulfilled; and we notice that it is independent of the battery resistance.

The resistance of the battery has been supposed to be of finite magnitude compared with the other resistances in the bridge. If, however, the resistance of the battery be indefinitely large compared with the sum of the four resistances in the bridge, then the condition

$$E = \frac{(a+b)(c+d)}{a+b+c+d}$$

holds universally, *i. e.* is independent of whether balance is nearly established or not. (Such a case could, of course, only occur in the physical laboratory.)

Again, if the battery resistance be indefinitely small compared with the sum of the four resistances in the bridge (as in measuring the "insulation" of a well-insulated telegraph-wire), we see that the condition

$$E = \frac{ac}{a+c} + \frac{bd}{b+d}$$

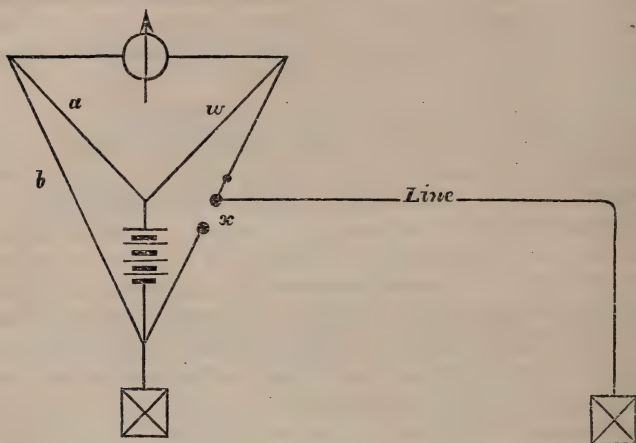
is universally true, *i. e.* is independent of whether balance is nearly established or not.

Of course when balance is nearly established the two conditions become identical.

Bridges for testing telegraph-lines are almost invariably wrongly arranged. It can be proved* that when the galvano-

* See Clerk Maxwell's 'Electricity and Magnetism,' Art. 348.

meter resistance is greater than the battery resistance (a condition which should always be fulfilled in testing telegraph-lines), the galvanometer should be made to connect the junction of the two greater resistances with that of the two lesser, thus :—



Now, considering the question of what resistance practically to give a galvanometer intended to be used in a Wheatstone's bridge for testing telegraph-lines, the condition of maximum sensibility becomes

$$g = \frac{(a+w)(b+x)}{a+b+w+x}.$$

Supposing the branches a and b to each consist of three coils of 10, 100, and 1000 ohms resistance respectively, and w to be variable between 1 ohm and 10,000 ohms, and putting*

$$\begin{aligned} a &= 10 \text{ ohms} \\ b &= 1000 \text{ „} \\ w &= 5500 \text{ „} \\ x &= 550,000 \text{ „} \end{aligned}$$

then we have approximately

$$g = 5500 \text{ ohms.}$$

Government Telegraph Department,
Electrician's Office, Alipore,
November 21, 1873.

* See Mr. Schwendler's paper in the Philosophical Magazine for May 1866.

V. *Further Discussion of the Analytical Principles of Hydrodynamics, in Reply to Mr. Moon.* By Professor CHALLIS, M.A., LL.D., F.R.S., F.R.A.S.*

IN vol. xxxvi. of the Philosophical Magazine (p. 117), and in the Number for September 1873 (p. 247), Mr. Moon has adduced an argument which, as expressed in the later publication, is as follows:—"For motion in one direction it is axiomatic that we must have in *every* fluid, in *every* case of motion,

$$p=f_1(x, t), \quad \rho=f_2(x, t), \quad v=f_3(x, t);$$

whence it follows that, for every value of x and t for which each of the foregoing equations represents a substantive relation between the variables, we shall have $p=\text{funct.}(\rho, v)$."

I have admitted the truth and the comprehensiveness of this conclusion, considering it to be a legitimate inference from the axiom that all physical relations expressible by functions of space and time are comprehended by the processes of abstract calculation (see Phil. Mag. for August 1873, pp. 160 & 165).

Also in my reply in the October Number (p. 310), I have extended the argument to space of three dimensions, and obtained the general equation $F(p, \rho, u, v, w)=0$.

I have, besides, ascertained that the values of p, ρ, v obtained by Mr. Moon in vol. xxxvi. (p. 124), and reproduced in the Number for December 1873 (p. 448), satisfy the differential equation

$$\frac{d^2y}{dt^2} + \frac{1}{D} \frac{dp}{dx} = 0. \quad . \quad . \quad . \quad . \quad . \quad (1)$$

Having made these statements, I am prepared to admit that Mr. Moon has rightly urged (in the December Number, p. 447), in opposition to an assertion I had made, that the assigning of a relation between ρ and v in the equation $p=f(\rho, v)$ does not simply define the fluid, but imposes also conditions on the motion existing in it, and that an analogous remark applies to the equation $F(p, \rho, u, v, w)=0$. Let us, however, inquire what inferences relative to this point are deducible from Mr. Moon's equations.

By a solution of the equation (1) he obtains for determining p the equation

$$p = -\frac{\alpha^2}{\rho} + \chi\left(v + \frac{\alpha}{\rho}\right),$$

α being an arbitrary constant, and the form of the function χ being arbitrary. Putting on the left-hand side of this equation $f(\rho, v)$ for p , it will be seen, since $\chi\left(v + \frac{\alpha}{\rho}\right)$ is arbitrary in form

* Communicated by the Author.

and value, that v is an arbitrary function of ρ , and, by consequence, on substituting for v in $f(\rho, v)$, that p is also an arbitrary function of ρ . Hence, by Mr. Moon's own reasoning, there may be an *unlimited* number of different relations between p and ρ , each defining a different fluid; and evidently among these the ordinary relation $p = a^2 \rho$ must be included.

Again, since $v = \frac{dy}{dt}$ and $\rho = D \left(\frac{dy}{dx} \right)^{-1}$, and it has just been shown that v is an arbitrary function of ρ , it follows that

$$\frac{dy}{dt} = F \left(\frac{dy}{dx} \right). \quad \dots \dots \dots (2)$$

By differentiating equation (2) with respect to t , we obtain

$$\frac{d^2y}{dt^2} = \left(F \left(\frac{dy}{dx} \right) \right)^2 \frac{d^2y}{dx^2}, \quad \dots \dots \dots (3)$$

which equation has the same generality as that from which it was derived.

The equations (2) and (3) are given by Mr. Earnshaw at the commencement of a paper "On the Mathematical Theory of Sound," contained in the *Philosophical Transactions* (vol. cl. p. 133), which is chiefly devoted to the discussion of the particular case in which, $\frac{dy}{dx}$ being always equal to $\frac{D}{\rho}$, we have

$$\frac{d^2y}{dx^2} \cdot \frac{d^2y}{dt^2} = a^2 \frac{d^2y}{dx^2}, \quad \dots \dots \dots (4)$$

this equation being that which (3) becomes on the hypothesis of Boyle's law. Now the equation (1) is identical with the equation (4) on the same hypothesis. For since $\rho = D \left(\frac{dy}{dx} \right)^{-1}$, and $p = a^2 \rho$, it follows that

$$\frac{dp}{dx} = a^2 \frac{d\rho}{dx} = -D \cdot \frac{\frac{d^2y}{dx^2}}{\frac{dy}{dx}^2};$$

whence by substituting for $\frac{dp}{dx}$ in (1) the equation (4) results.

At the end of his paper Mr. Earnshaw briefly indicates the mode of treatment of the case in which

$$\frac{d^2y}{dt^2} = f_1 \left(\frac{dy}{dx}, \frac{dy}{dt} \right) \cdot \frac{d^2y}{dx^2},$$

assuming, as before, that $\frac{dy}{dt} = F \left(\frac{dy}{dx} \right)$. I shall now show that

this form of the equation (3) is derivable from Mr. Moon's equation $p=f(\rho, v)$ and the equation (1). From the value of p we get by differentiation

$$\frac{dp}{dx} = \frac{df}{d\rho} \frac{d\rho}{dx} + \frac{df}{dv} \frac{dv}{dx},$$

f being put for shortness' sake for $f(\rho, v)$. Hence, since

$$\frac{dp}{dx} = -D \frac{\frac{d^2y}{dx^2}}{\frac{dy}{dx}}, \text{ and } \frac{dv}{dx} = \frac{d^2y}{dx dt} = F' \left(\frac{dy}{dx} \right) \frac{d^2y}{dx^2},$$

it follows, by substituting $\frac{D}{\rho}$ for $\frac{dy}{dx}$, that

$$\frac{dp}{dx} = -\frac{df}{d\rho} \frac{\rho^2}{D} \frac{d^2y}{dx^2} + \frac{df}{dv} F' \left(\frac{D}{\rho} \right) \frac{d^2y}{dx^2}.$$

Consequently, by substituting in (1),

$$\frac{d^2y}{dt^2} = \frac{1}{D} \left(\frac{df}{d\rho} \cdot \frac{\rho^2}{D} - \frac{df}{dv} \cdot F' \left(\frac{D}{\rho} \right) \right) \frac{d^2y}{dx^2}.$$

But the partial differential coefficients $\frac{df}{d\rho}$ and $\frac{df}{dv}$ are functions of ρ and v . Hence this equation may be put under the form

$$\frac{d^2y}{dt^2} = \psi(\rho, v) \frac{d^2y}{dx^2},$$

and is therefore equivalent to the above-cited equation,

$$\frac{d^2y}{dt^2} = f_1 \left(\frac{dy}{dx}, \frac{dy}{dt} \right) \frac{d^2y}{dx^2}.$$

Since $\frac{dy}{dt} = F \left(\frac{dy}{dx} \right)$, the last equation may be assumed to be identical with the equation (3), which, as Mr. Earnshaw has justly remarked, "can be made to coincide with any dynamical equation in which the ratio of $\frac{d^2y}{dt^2}$ to $\frac{d^2y}{dx^2}$ can be expressed in terms of $\frac{dy}{dx}$."

It has thus, I think, been sufficiently proved that Mr. Moon's hydrodynamical researches are founded on differential equations which are really the same as those employed by Mr. Earnshaw, and differ only in the process of investigation and form of expression, and should consequently lead to the same results. This identity indicates that the two mathematicians have argued correctly, although by different processes, from the *same* princi-

ples, which, as certainly in the one case as in the other, are *the commonly received principles of hydrodynamics*. It is therefore logically impossible that any argument against those principles can be drawn from Mr. Moon's reasoning or his results, inasmuch as the reasoning has no other basis to stand upon.

I think it right to add that I have advanced the foregoing argument only as an *argumentum ad hominem*, intended to show that, even admitting Mr. Moon's premises and his reasoning, no conclusion can be drawn therefrom contradictory to the principles of hydrodynamics. My reason for making this statement here is that I have repeatedly, in this Journal and elsewhere, asserted that I cannot accept either the equation (1) or the equation (4) as a true hydrodynamical equation—not because the principles on which these equations rest are untrue, but because they require to be supplemented. At the same time I can admit the truth and the importance of the equations $p=f(\rho, v)$ and $F(p, \rho, u, v, w)=0$, considering them to be necessary for completing the analytical principles of hydrodynamics. Experiment can establish the truth of the equation $p=a^2\rho$ only for fluid at rest, whereas by means of these equations it can be shown to be legitimate to assume the same equation to hold good for fluid in motion.

Having given these explanations I think it needless to pursue the subject at greater length, and accordingly shall decline entering upon any further discussion.

Cambridge, December 16, 1873.

VI. *On the Electromotive and Thermoelectric Forces of some Metallic Alloys in contact with Copper.* By A. F. SUNDELL, Docent at the University of Helsingfors*.

THE remarkable thermoelectric properties of metallic alloys discovered by the investigations of Seebeck, Rollmann, and other physicists permitted it to be supposed that interesting phenomena must also occur in relation to the power of these bodies, in contact with each other or with unmixed metals, to act in exciting electricity. At the desire of Professor Edlund, therefore, who has determined by a new method the electromotive forces of a greater number of metals in contact with copper, and compared them with the thermoelectric forces†, I under-

* Translated from a separate impression, communicated by the Author, from Poggendorff's *Annalen*, vol. cxlix. pp. 144–170.

† *Kongl. Svenska Vet.-Akademiens Handlingar*, vol. ix. (1871) No. 14; *Phil. Mag.* Jan. 1871, p. 18; *Pogg. Ann.* vol. cxliii. pp. 404 & 534. A preliminary investigation will be found in *Öfversigt af K. Vet.-Akad. Förhandlingar* 1870, p. 3; *Pogg. Ann.* vol. cxl. p. 435.

took, in the beginning of the year 1872, a similar investigation of various alloys. The present memoir contains the results, the investigation having been carried out in the physical laboratory of the Royal Swedish Academy of Sciences, Stockholm.

1. M. Edlund's method is based on the proposition, deduced from the mechanical theory of heat, that a galvanic current going through an electromotor calls forth in it an absorption or production of heat proportional to its electromotive force, according as the current is in direction the same as or opposite to that generated by the electromotor itself*. Hence the relative quantity of the electromotive force is obtained by measuring the amount of heat absorbed or produced. The phenomenon also indicates, as is readily seen, the direction of the current excited by the electromotor.

The air-thermometer constructed by Professor Edlund for the measurement of the quantities of heat in question is fully described in his memoir; consequently it will be sufficient here to mention only the principal parts of this apparatus. Two perfectly equal cylinders of thin sheet copper communicate through a horizontal glass tube of 2.5 millims. internal diameter. Into both cylinders, through open tubes which enter the middle points of their ends, the same combination of two metals in the shape of wires or rods is inserted in such wise that the place of contact is in the centre of the cylinder. The tubes are lined internally, to half their length, with wood, to prevent contact between the cylinder and the wire. Communication between the outer air and that of the cylinder is then completely cut off by pouring into the tubes a melted mixture of equal parts by weight of wax and rosin. The two wires are inserted in the circuit of a galvanic current in such a manner that, when the current in one of the wires goes from metal A to metal B, the second wire is passed through by the current in the opposite direction, from B to A. Hence arises at the one place of contact a production, at the other an absorption, of heat. The trifling difference hereby produced between the temperatures of the air in the cylinders occasions the displacement of a column a few centimetres long of liquid in the tube, a mixture of alcohol and water of sp. gr. 0.9 which acts as an index. If the current be reversed, a cooling ensues in the cylinder in which there was previously a heating, and *vice versa*, by which the index is moved towards the opposite side. The displacement towards the one side or the other ceases as soon as equilibrium is restored between the heat given off by each cylinder through radiation and conduction to

* This proposition was first advanced by Professor Edlund (*Æfv. af K. Vet.-Akad. Förh.* 1869, p. 457; *Pogg. Ann.* vol. cxxxvii. p. 474; *Phil. Mag.* S. 4. vol. xxxviii. p. 263).

the outer air and that developed by the current in the wires. The positions in which the index becomes stationary are read off a millimetre-scale fixed to the glass tube. The magnitude of the deflection is given by the difference between the numbers read off. A displacement of the liquid one scale-division corresponds to a difference of temperature of $0^{\circ}\cdot002$ Cels.

Each combination was tried with three different intensities of current. From the deflections the quantity α was calculated, which is proportional to the quantity of heat absorbed or produced with the unit of intensity ($\tan 45^{\circ}$), according to the equation

$$\alpha s = (\sqrt{\beta s^2 + 1})t, \quad . \quad . \quad . \quad . \quad . \quad (1)$$

in which β is a constant proportional to the galvanic resistance of the wire in the cylinder, s the current-intensity, and t the deflection. The constant β is obtained from the deflections t and t_1 at the intensities s and s_1 , by the formula

$$\beta = \frac{(t_1 s + t s_1)(t_1 s - t s_1)}{s^2 s_1^2 (t + t_1)(t - t_1)}. \quad . \quad . \quad . \quad . \quad . \quad (2)$$

The combination of each two of the three current-intensities gave a value of β ; in the calculation of α the arithmetic mean of the three values were inserted in equation (1).

Professor Edlund, in nearly all his experiments, made use of two copper cylinders silvered on the outer side. These cylinders proved now to be no longer air-tight; and therefore in the present investigation two others, unsilvered, also employed by Edlund in some experiments, were used.

Waiting till the displacement ceases, before reading-off the position of the column of liquid, makes the labour very tedious. This method is not always practicable, because the index has mostly a motion of its own, independent of the temperature-variation to be measured, the cause of which will be subsequently stated. Professor Edlund therefore made his experiments after the following method. The position of the liquid column was read off at the end of each three quarters of an hour; and then the current was reversed. The effect of the peculiar motion (which could be taken as uniformly accelerated or retarded) was eliminated in the following manner. The correct deflection, corresponding to the quantity of heat developed or which had vanished at the place of contact, was obtained from three deflections observed in immediate succession, by taking the arithmetic mean of the first and third, and then the mean of this mean and the second deflection. In order to save time, Professor Edlund performed the reading in some experiments every quarter of an hour. Although the temperature of the cylinders had not in so short a time arrived at equilibrium, yet the deflections thus ob-

tained may be employed, because the values of α which were calculated from them are, as Edlund has shown, proportional to those furnished by the readings after 45 minutes. By special experiments, which shall in due time be given, it was ascertained that there was this proportionality even when the unsilvered cylinders were employed. Hence in the present investigation, except where otherwise stated, the readings were everywhere taken every quarter of an hour.

As essential parts of the apparatus the double cylinders made of metal sheet and filled with water must be mentioned, the *mantles*, which protected the copper cylinders against the variations of temperature in the observation-room. Those employed by M. Edlund were made of zinc. During the experiments the oxidating action of the water had covered the inner sides of the mantles with a thick layer of hydrate of zinc, which obstructed the passage of the heat developed in the air-thermometer into the water. Thereby was produced an elevation of temperature in the annular layer of air between a cylinder and its mantle; and because this rise of temperature was not equally great for both cylinders, the column of liquid in the glass tube was put in the above-mentioned continuous motion towards one side. To remove this a means was tried which is indicated in M. Edlund's memoir. New mantles were procured, of the same dimensions as the old, but of sheet brass and electroplated with silver on the side presented to the water*. These were filled with distilled water. With this arrangement the continuous motion almost entirely disappeared; only towards the end of the investigation did it show itself in a slightly higher degree. The reason for it, however, did not lie in the mantles; for the displacement in question vanished again when an experiment was made with the silvered cylinders. Probably one of the unsilvered cylinders had undergone a change, so that it gave up the heat with greater or less velocity than the other. On account of the continuous displacement, the deflections given in the following are in every case calculated, according to the method before-mentioned, from three deflections observed in succession.

2. The galvanic current was generated by a pile of from two to four Bunsen's elements; and in the circuit, besides the wires in the cylinders, a rheostat, a tangent-compass, and a commutator were inserted. The alloys, except the German silver, were cast in a mould of gypsum, in cylindrical rods of about 16 centims. length and 2 millims. thickness. These were soldered with tin to copper wires 1 millimetre thick, which had been drawn out of chemically pure, galvanically precipitated copper.

* The entire apparatus in this improved form is delivered by M. Sörensen, of Stockholm, at the price of 110 thalers Prussian currency.

In the alloys commercial (impure) metals were employed. During the investigation it was observed that both the electromotive and the thermoelectric force diminished as the time proceeded. This diminution, especially remarkable immediately after casting, depended doubtless on physical (perhaps also chemical) molecular changes in the alloys. As the electromotive force was usually determined immediately after the casting, but the thermoelectric not till some days subsequently, one could not be sure that both determinations corresponded to the same molecular state of the alloys. It was consequently necessary with most of the alloys to repeat the experiments. This later investigation was effected perhaps six weeks after the first; and the same pair of wires was employed. As it could be assumed that at the end of this time the molecular changes had for the most part ceased, and since also the determination of the thermoelectric force was now taken immediately after the ascertaining of the electromotive force, the values of the two forces thus obtained correspond to the same molecular state and are comparable with each other.

3. The determination of the *thermoelectric* force was effected in the following manner. The copper wire which was soldered to the alloy was bent, near the soldering, twice at right angles, and so made parallel to the alloy at a distance of about 2 centims. The soldering was inserted in a test-tube, which was immersed in cold water, whereby the place of contact was cooled $7-10^{\circ}$ C. below the temperature of the room ($15-18^{\circ}$ C.). The free ends of the wires were joined to the ends of the galvanometer-wire by means of binding-screws; these were surrounded by a wooden case, the ends of which were perforated for the wires to pass through. The difference of temperature of the places of contact was ascertained by two exactly similar very sensitive thermometers. The bulb of one of them was put close to the place of contact in the test-tube, that of the other near the binding-screws in the box; and then the mouth of the test-tube and the perforations of the box were stopped with cotton-wool, in order to prevent currents of air. After the contacts had acquired the temperature of the air surrounding them, the two thermometers and the deflection of the galvanometer were read off simultaneously several times at brief intervals. The galvanometer was furnished with the usual mirror-reading. The conducting-power, measured by means of an inducing magnet, could be modified by a rheostat interpolated in the circuit.

4. We come now to the narration of the observations. The experiments made at the *later* determination of the electromotive and thermoelectric forces will be given in full. Of the experiments first made the results only will be stated, in order to render

conspicuous the influence of the molecular changes. In the denomination of the combinations we shall always place first the name of that substance to which the positive galvanic current at the place of contact must go in order to produce a cooling. The electromotive force of the combination generates therefore a current which at the contact-place goes from the last-named to the first-named body. The same direction always resulted also for the thermoelectric current at the hotter place of contact. We may therefore regard the first-named body as the electropositive, and the last-named as the electronegative constituent of the combination.

Iron-copper.

For the purpose of being able to compare the electromotive force of the alloys with that of the unmixed metals, the combinations iron-copper and copper-bismuth (in fact, the wires of these combinations already used by M. Edlund) were first tried. The determination of the *electromotive* force of the combination *iron-copper* gave the following result:—

Experiment 1. Current-intensity $s = \tan 29^\circ 6'5$.

Deflections.

33·65

33·72

33·22

32·49

Mean . . 33·27

Experiment 2.
 $s = \tan 19^\circ 30'5$.

Deflections.

23·57

23·50

23·50

23·70

Mean . 23·57

Experiment 3.
 $s = \tan 8^\circ 35'$.

Deflections.

11·37

12·22

12·20

Mean . 11·9

The mean value of $\beta = 2\cdot911$. With this we obtain for α the following values:—

From Exp. 1, $\alpha = 82\cdot42$

„ 2, $\alpha = 77\cdot74$

„ 3, $\alpha = 81\cdot62$

Mean . . $\alpha = 80\cdot59$

At the end of the entire investigation the iron-copper combination was tested afresh to ascertain what changes might have

taken place in the apparatus. There was then obtained $\beta = 2.976$ and

$$\begin{array}{rcl} \alpha = 86.87 & \text{with } s = \tan 31^\circ & 1 \\ \alpha = 81.27 & ,, & s = \tan 20^\circ 18' \\ \alpha = 84.24 & ,, & s = \tan 10^\circ 13' \end{array}$$

$$\text{Mean } \alpha = 84.13$$

The mean value of the two determinations is $\alpha = 82.36$. Professor Edlund obtained $\alpha = 130.99$. The ratio of these values is 1.59.

The electromotive force of this combination was also determined from readings with intervals of 45 minutes.

As the result of these experiments I obtained $\beta = 2.156$ and

$$\begin{array}{rcl} \alpha = 106.4 & \text{with } s = \tan 30^\circ & 21' \\ \alpha = 100.5 & ,, & s = \tan 19^\circ 55' \\ \alpha = 104.6 & ,, & s = \tan 10^\circ 28' \end{array}$$

$$\text{Mean } \alpha = 103.83$$

The ratio of the two values of α obtained according to the two different methods is 1.26.

The determination of the *thermoelectric* force in the way above given furnished the following result:—

Experiment 4.—Pair of wires No. 1.

Temperature-differences of the contacts.	Thermoelectric-current deflections.	Conducting-power (inducing-magnet deflections).
10.2	111.2	198.0
10.1	109.4	198.0
9.9	108.4	197.6
9.8	107.4	
Mean ... 10.0	109.1	197.9

Experiment 5.—Pair of wires No. 2.

8.9	94.0	198.4
8.7	91.0	199.0
8.6	88.8	197.6
8.4	86.6	
Mean ... 8.64	90.1	198.3

In all the following experiments we shall take the deflections, reduced to the temperature-difference 10°C. and the conducting-power 160, as relative measures of the thermoelectric forces. The magnitude of this force of the combination iron-copper is

consequently,

$$\begin{array}{rcl} \text{for the pair of wires No. 1,} & = & 88\cdot20 \\ \text{,, ,, No. 2,} & = & 84\cdot04 \\ \text{Mean . . .} & = & 86\cdot12 \end{array}$$

The quotient obtained by the division of the value obtained by M. Edlund (146·18) by this is 1·70.

The values above obtained of the thermoelectric and electromotive forces have the ratio $\frac{86\cdot12}{82\cdot36} = 1\cdot05$. If this number be multiplied by the reduction-factor $\frac{1\cdot70}{1\cdot59} = 1\cdot07$, the number 1·12 is obtained, which is exactly equal to the numerical proportion of the two forces determined by Professor Edlund.

Copper-bismuth.

Electromotive Force.

Experiment 6. $s = \tan 27^{\circ} 35'$.	Experiment 7. $s = \tan 12^{\circ} 55'$.	Experiment 8. $s = \tan 6^{\circ} 45'$.
Deflections.	Deflections.	Deflections.
173·25	85·70	47·85
172·70	85·50	48·00
172·70	85·10	48·05
Mean...172·88	85·43	47·97

These experiments give $\beta = 2\cdot794$ and

$$\begin{array}{rcl} \alpha = 439\cdot33 & \text{from Exp. 6,} \\ \alpha = 398\cdot95 & \text{,, 7,} \\ \alpha = 413\cdot14 & \text{,, 8.} \end{array}$$

$$\text{Mean . . . } \alpha = 417\cdot14$$

Professor Edlund found $\alpha = 783\cdot10$. The ratio $\frac{783\cdot10}{417\cdot14} = 1\cdot88$ is greater than that corresponding to the combination iron-copper. Since it may be assumed that the molecular state of the iron-copper wire is pretty constant, this signifies that the rod of crystalline bismuth had undergone some molecular change since its examination by Professor Edlund. This alteration had induced a *diminution* of the electromotive force.

The *thermoelectric* experiments gave the following result:—

Experiment 9.—No. 1.

Temperature-differences.	Deflections.	Conducting-power.
7·8	231	111·4
7·6	228	112·0
Mean ... 7·7	229·5	111·7
Experiment 10.—No. 2.		
6·6	226·2	111
5·5	222·0	111
6·4	219·0	
Mean ... 6·5	222·4	111

The deflections become by reduction :—

For No. 1, = 426·93

„ No. 2, = 493·19

Mean . . . = 460·06

Professor Edlund obtained for the thermoelectric force the value 835·10, the ratio of which to 460·06 equals 1·82. For the iron-copper combination this ratio was only 1·70. The thermoelectric force of the copper-bismuth combination had therefore, like its electromotive force, *diminished*.

Alloy No. 1 and Copper.

The alloy consisted of 8 parts bismuth and 1 part tin. The experiments to determine the *electromotive* force gave :—

Experiment 11. $s = \tan 18^\circ 24'$	Experiment 12. $s = \tan 12^\circ 33'$	Experiment 13. $s = \tan 7^\circ 32'$
Deflections.	Deflections.	Deflections.
60·85	41·65	29·35
60·45	44·85	29·35
58·60	43·00	29·75
59·05		29·30
Mean ... 59·74	43·17	29·44

From these we get $\beta = 7·178$ and

$\alpha = 240·56$ from Exp. 11,

$\alpha = 225·79$ „ 12,

$\alpha = 236·18$ „ 13.

Mean . . . $\alpha = 234·18$

In the first determination β was = 6.336, and

$$\alpha = 255.46 \text{ with } s = \tan 18^{\circ} 22'$$

$$\alpha = 235.28 \quad ,, \quad s = \tan 12^{\circ} 05'$$

$$\alpha = 249.89 \quad ,, \quad s = \tan 5^{\circ} 32'$$

$$\text{Mean} \quad . \quad . \quad \alpha = 246.88$$

The electromotive force of the alloy appears therefore to *diminish* with time.

The *thermoelectric* experiments gave the following result :—

Experiment 14.—No. 1.

Temperature-differences.	Deflections.	Conducting-power.
10.0	143.6	93.4
9.9	141.4	93.2
9.7	140.0	93.2
9.6	138.2	
Mean ... 9.8	140.8	93.3
Experiment 15.—No. 2.		
10.8	142.8	93.2
10.7	141.6	94.0
10.6	141.0	93.8
10.5	139.4	
Mean ... 10.65	141.2	93.7

The deflections, reduced, are :—

$$\text{For No. 1,} = 246.38$$

$$,, \quad 2, = 226.39$$

$$\text{Mean} \quad . \quad . = 236.39$$

From the first experiments it follows that :—

$$\text{No. 1. Reduced deflection} = 252.98$$

$$\text{No. 2.} \quad ,, \quad ,, = 229.53$$

$$\text{Mean} \quad . \quad . = 241.26$$

Consequently the thermoelectric, like the electromotive force, proved somewhat *smaller* in the later experiments than in those first made.

Alloy No. 2 and Copper.

The elements of this alloy were 4 parts bismuth and 1 part tin. The *electromotive* force was ascertained by the following experiments :—

Experiment 16. $s = \tan 19^\circ 17'$.	Experiment 17. $s = \tan 12^\circ 18'$.	Experiment 18. $s = \tan 6^\circ 0'$.
Deflections.	Deflections.	Deflections.
39.1	25.75	14.65
38.4	25.85	14.50
38.6	25.55	14.55
Mean ... 38.7	25.72	14.57

The value of $\beta = 4.815$; with this we obtain

$$\alpha = 139.45 \text{ from Exp. 16.}$$

$$\alpha = 130.77 \quad \text{,,} \quad 17.$$

$$\alpha = 142.26 \quad \text{,,} \quad 18.$$

$$\text{Mean . . } \alpha = 137.49$$

Experiments were also made with intervals of 45 minutes, and furnished the following result:— $\beta = 4.991$ and

$$\alpha = 178.52 \text{ with } s = \tan 21^\circ 12'$$

$$\alpha = 176.90 \quad \text{,,} \quad s = \tan 15^\circ 8'$$

$$\alpha = 178.62 \quad \text{,,} \quad s = \tan 7^\circ 33'$$

$$\text{Mean . . } \alpha = 178.01$$

The ratio of the two values of α is 1.29, which is, within the limits of errors of observation, equal to the ratio found with the iron-copper combination. Hence the method with shortened time between two successive readings gives a sufficiently exact result.

With the *thermoelectric* experiments the following deflections were obtained:—

Experiment 19.—No. 1.

Temperature-differences.	Deflections.	Conducting-power.
10.5	188.5	213.2
10.3	185.1	213.4
10.2	179.7	213.4
10.0	178.3	
Mean... 10.23	182.9	213.3
Experiment 20.—No. 2.		
11.1	232.4	212.3
11.0	230.6	212.8
10.9	228.4	212.4
10.8	225.0	
Mean ... 10.95	229.1	212.7

The deflections, reduced, are:—

$$\begin{aligned} \text{For No. 1,} &= 134.11 \\ \text{" No. 2,} &= 157.38 \\ \text{Mean . . .} &= 145.75 \end{aligned}$$

From the first determination of the electromotive force, made immediately after the casting, I obtained $\beta = 4.008$ and

$$\begin{aligned} \alpha &= 184.8 \text{ with } s = \tan 26^{\circ} 34' \\ \alpha &= 162.7 \quad ;, \quad s = \tan 16^{\circ} 3' \\ \alpha &= 176.4 \quad ,, \quad s = \tan 6^{\circ} 37' \end{aligned}$$

$$\text{Mean . . } \alpha = 174.63$$

Hence the electromotive force directly after the casting was sensibly greater than in the later experiments. About ten days after the casting the reduced deflections of the thermoelectric current were:—

$$\begin{aligned} \text{For No. 1,} &= 134.91 \\ \text{,, No. 2,} &= 165.83 \\ \text{Mean . . .} &= 150.37 \end{aligned}$$

This value is but little greater than that obtained in the later experiments. Probably the molecular change which brought about the diminution of the electromotive and thermoelectric forces had for the most part already commenced when the first thermoelectric experiments were made.

Alloy No. 3 and Copper.

The constituents of this alloy were 2 parts bismuth and 1 part tin.

The *electromotive force*.—

Experiment 21. $s = \tan 19^{\circ} 12'$.	Experiment 22. $s = \tan 13^{\circ} 23'$.	Experiment 23. $s = \tan 7^{\circ} 40'$.
Deflections.	Deflections.	Deflections.
12.00	8.70	6.00
13.85	8.90	6.75
13.65	9.65	6.20
12.35		
Mean ... 12.96	9.08	6.32

We obtain $\beta = 8.275$ and

$$\begin{aligned} \alpha &= 52.68 \text{ from Exp. 21.} \\ \alpha &= 46.25 \quad ,, \quad 22. \\ \alpha &= 50.35 \quad ,, \quad 23. \\ \text{Mean . . } \alpha &= 49.76 \end{aligned}$$

The thermoelectric force.—

Experiment 24.—No. 1.

Temperature-differences.	Deflections.	Conducting-power.
10.4	65.8	196.0
10.3	64.8	196.2
10.1	63.8	196.0
Mean... 10.27	64.8	196.1

Experiment 25.—No. 2.		
10.3	65.2	196.8
10.2	64.6	196.2
10.0	63.8	196.2
Mean... 10.17	64.53	196.4

The deflections become when reduced :—

For No. 1, = 51.48

„ No. 2, = 51.69

Mean . . = 51.59

The diminution of the electromotive force of this alloy was particularly great ; for the first determination gave $\alpha = 94.34$, a value nearly twice as much as the one found above. The thermoelectric force was also at the commencement much greater ; for the deflections became when reduced :—

For No. 1, = 78.76

„ No. 2, = 75.08

Mean . . = 76.92

In order to verify the presence of the alteration in question, special experiments were made with this alloy. Three rods were cast at the same time, and soldered to copper wires. Two of these pairs of wires were inserted in the air-thermometer. In the following days the electromotive force was determined, and also simultaneous thermoelectric experiments made with the third pair of wires.

Experiment 26, on the 28th of February, directly after the casting.—With the current-intensity $s = \tan 20^\circ$ the mean deflection 34.43 was obtained. The reduced deflection of the thermoelectric current was 120.24.

Experiment 27, on the same day, about 5 hours later.—Deflection with $s = \tan 20^\circ 10'$, 30.33. Reduced deflection of the thermoelectric current = 113.39.

Experiment 28, on the 29th Feb.—To ascertain the value of β , deflections were taken with two different intensities of the current. We obtained:—

$$\begin{array}{lll} \text{With } s = \tan 19^\circ 53' & \text{the deflection } 27\cdot15, \\ \text{,, } s = \tan 7^\circ 45' & \text{,, } & 12\cdot80, \end{array}$$

whence $\beta = 5\cdot600$. This experiment gave the thermoelectric force = $103\cdot37$.

Experiment 29, on March 6.—With $s = \tan 20^\circ 13'$ the deflection in the air-thermometer was $22\cdot35$. Thermoelectric force = $84\cdot27$.

Experiment 30, March 9.—Deflection = $19\cdot08$ when $s = \tan 19^\circ 35'$. Thermoelectric force = $74\cdot61$.

The following Table makes evident the constant diminution of both forces. The first column gives the day of the experiment; the second contains the values of α which have been calculated, with the value of $\beta = 5\cdot6$, from Exp. 26–30. The reduced deflections of the thermoelectric current are given in the third column; the fourth is obtained by division of the numbers in the third column by those in the second.

	Electromotive force.	Thermoelectric force.	Ratio.
February 28.	124·85	120·24	0·96
„ 28.	109·42	113·39	1·03
„ 29.	98·81	103·37	1·05
March 6.	80·51	84·27	1·05
„ 9.	70·11	74·61	1·06

Consequently both forces diminish in nearly the same proportion. The alteration proceeded most quickly directly after the casting, the diminution being already sensible after a few hours. It might be supposed that the galvanic current (which, it is well known, calls forth both temporary and permanent changes in the conducting-wire) is the cause of the phenomenon here observed. That this is not the case, however, is shown by the experiments with the wires first cast, in which the change had taken place although they had lain several weeks unused, as well as Experiments 26–30, in which one of the pairs of wires had not been exposed to the influence of any stronger current than the thermoelectric current produced by the cooling of the place of soldering. It must also be remarked that the diminution of the electromotive force cannot be ascribed to any change of structure of the copper wire, because this had been drawn a very long time before the experiments.

Alloy No. 4 and Copper.

The constituents were 12 parts bismuth and 1 part tin.
The *electromotive* force.

Experiment 31. $s = \tan 19^\circ 28'$.	Experiment 32. $s = \tan 12^\circ 54'$.	Experiment 33. $s = \tan 7^\circ 50'$.
Deflections.	Deflections.	Deflections.
67·95	47·15	34·3
68·25	50·25	32·7
.....	47·00	32·6
Mean ... 68·10	48·13	33·2

Calculation gives $\beta = 6·866$ and

$$\alpha = 262·61 \text{ from. Exp. 31,}$$

$$\alpha = 245·09 \quad \text{,,} \quad 32,$$

$$\alpha = 256·52 \quad \text{,,} \quad 33.$$

$$\text{Mean . . . } \alpha = 254·74$$

The *thermoelectric* force.—

Experiment 34.—No. 1.

Temperature-differences.	Deflections.	Conducting-power.
9·8	283·0	170·0
9·7	277·0	170·8
9·5	273·4	170·2
Mean ... 9·67	277·8	170·3

Experiment 35.—No. 2.

7·7	220·0	168·0
7·6	217·4	169·4
7·5	215·2	168·6
Mean ... 7·6	217·53	168·7

Reduced deflection:—For No. 1, = 269·91

$$\text{,,} \quad 2, = 271·46$$

$$\text{Mean . . .} = 270·69$$

The first determination gave the electromotive force = 256·69 and the thermoelectric = 266·89, values which differ but little from those obtained later. In order to learn whether this alloy undergoes in the course of time a similar change as the preceding, it was recast and its thermoelectric force investigated. Immediately after casting the reduced deflection was 297·63; a day

later, 292·63; and six days after casting, 274·12. These experiments show distinctly that the thermoelectric force (and consequently also the electromotive force) of this alloy *diminishes* with time.

Copper and Alloy No. 5.

The constituents were 32 parts bismuth and 1 part antimony. The *electromotive* force.—

Experiment 36. $s = \tan 19^{\circ} 20'$.	Experiment 37. $s = \tan 12^{\circ} 45'$.	Experiment 38. $s = \tan 7^{\circ} 0'$.
Deflections.	Deflections.	Deflections.
86·48	57·60	36·0
88·15	57·55	35·9
.....	58·00	34·2
Mean ... 87·32	57·72	35·37

This gives $\beta = 4·213$ and

$$\alpha = 306·70 \text{ with } s = \tan 19^{\circ} 20'$$

$$\alpha = 281·25 \quad ,, \quad s = \tan 12^{\circ} 45'$$

$$\alpha = 297·08 \quad ,, \quad s = \tan 7^{\circ} 0'$$

$$\text{Mean} \quad . \quad \alpha = 295·01$$

In the first determination there was obtained $\beta = 6·083$ and

$$\alpha = 307·61 \text{ with } s = \tan 16^{\circ} 14'$$

$$\alpha = 290·19 \quad ,, \quad s = \tan 8^{\circ} 35'$$

$$\alpha = 298·64 \quad ,, \quad s = \tan 4^{\circ} 6'$$

$$\text{Mean} \quad . \quad \alpha = 298·81$$

The two values of α do not differ much from each other.

The *thermoelectric* force :—

Experiment 39.—No. 1.

Temperature-differences.	Deflections.	Conducting-power.
10·5	217·8	163·6
10·3	214·8	112·8
10·2	213·8	113·4
10·0	210·8	
Mean ... 10·25	214·3	113·3

The deflection, reduced, becomes 295·24.

The experiment with the pair of wires No. 2 could not be carried out, because in the preparation of the experiment it was injured at the joint. In the first determination values of the thermoelectric force but little differing were obtained for the two

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wires, viz.:—for No. 1 the value 313·06; for No. 2, 309·62; mean, 311·34. Hence we can take the thermoelectric forces of the two wires as equal also in the later determination. The difference between the numbers 295·24 and 311·34 is not of importance.

Copper and Alloy No. 6.

This alloy was obtained by melting together 32 parts bismuth and 3 parts antimony.

The *electromotive force*.—

Experiment 40. $s = \tan 18^\circ 18'$.	Experiment 41. $s = \tan 13^\circ 10'$.	Experiment 42. $s = \tan 7^\circ 28'$.
Deflections.	Deflections.	Deflections.
149·45 146·05 147·40	110·6 109·8 109·3	66·55 69·60
Mean ... 147·63	109·9	68·08

From these experiments $\beta = 4·306$ and

$$\begin{aligned} \alpha &= 541·40 \text{ with } s = \tan 18^\circ 18', \\ \alpha &= 522·21 \quad \text{,,} \quad s = \tan 13^\circ 10', \\ \alpha &= 538·32 \quad \text{,,} \quad s = \tan 7^\circ 28'. \end{aligned}$$

$$\text{Mean} \quad . \quad \alpha = 533·98$$

The *thermoelectric force*.—

Experiment 43.—No. 1.

Temperature-Differences.	Deflections.	Conducting-power.
7·0 6·7 6·4	231 225 221	79·8 80·0 80·2
Mean ... 6·7	225·67	80·0

Experiment 44.—No. 2.

Temperature-Differences.	Deflections.	Conducting-power.
7·0 6·9 6·8	246·0 243·2 240·0	82·2 82·0 81·6
Mean ... 6·9	243·07	81·9

Reduced deflection:—

$$\begin{aligned} \text{For No. 1,} &= 673·64 \\ \text{,, 2,} &= 688·23 \\ \text{Mean} \quad . \quad . &= 680·94 \end{aligned}$$

Eleven days previously the thermoelectric force was determined at 680·93, and the electromotive at 521·23. This alloy appears therefore to be as little variable in relation to its electromotive properties as the preceding bismuth-antimony. This was confirmed by a special thermoelectric experiment. Immediately after casting, the reduced deflection 613·87 was obtained, and five days afterwards the deflection 624·13. The difference does not amount to 2 per cent.; so that they may be regarded as not essentially different.

Copper and German Silver.

The copper wire was soldered to a German-silver wire of the same thickness (1 millim.). This combination was tried only once. The *electromotive force*.—

Experiment 45. $s = \tan 23^\circ 30'$.	Experiment 46. $s = \tan 15^\circ 16'$.	Experiment 47. $s = \tan 7^\circ 10'$.
Deflections.	Deflections.	Deflections.
32·60	21·75	12·05
32·70	21·95	12·05
32·35		12·15
Mean ... 32·55	21·85	12·08

The constant $\beta = 4·556$; and

$$\alpha = 102·13 \text{ from Exp. 45,}$$

$$\alpha = 92·65 \quad \text{,,} \quad 46,$$

$$\alpha = 99·47 \quad \text{,,} \quad 47.$$

$$\text{Mean } \alpha = 98·08$$

The *thermoelectric force*.—

Experiment 48.—No. 1.

Temperature-Differences.	Deflections.	Conducting-power.
11·9	158·8	210·2
11·4	151·8	210·0
11·3	150·8	209·8
11·1	149·6	
Mean ... 10·43	152·75	210·0
Experiment 49.—No. 2.		
11·0	153·2	209·2
10·6	145·4	210·6
10·3	138·2	
Mean ... 10·63	145·6	209·9

Reduced deflection :—

For No. 1, = 101·82

„ No. 2, = 104·41

Mean = 103·12

5. *Results of the Investigation.*—If the bodies investigated be arranged according to the magnitude of the electromotive and thermoelectric forces, in such wise that a preceding, electropositive, is opposed to a succeeding one, the following series are obtained, in which the numbers give the relative values of these forces on contact with copper. In the composition of the series, only the values obtained in the *later* determinations are taken into account.

	Electromotive force.	Thermoelectric force.
12 bismuth 1 tin . . .	254·74	270·69
8 „ 1 „ . . .	234·18	236·39
4 „ 1 „ . . .	137·49	145·75
Iron	82·36	86·12
2 bismuth 1 tin . . .	49·76	51·59
Copper	0	0
German silver	98·08	103·12
32 bismuth 1 antimony .	295·01	295·24
Bismuth	417·14	460·06
32 bismuth 3 antimony .	533·98	680·94

Consequently a metallic alloy, like the pure metals, takes the same position in the electromotive as in the thermoelectric series. Because the thermoelectric current at the hotter place of contact has the same direction as the galvanic current produced by the electromotive force in action there, *this force is augmented by the heating.* These results hold only for temperatures below +30° C., above which the temperature did not at any time ascend during the experiments.

If the numbers of the thermoelectric series be divided by the corresponding ones of the electromotive series the following are the quotients obtained :—

12 bismuth 1 tin, and copper . . .	1·10
8 bismuth 1 tin, and copper . . .	1·09
4 bismuth 1 tin, and copper . . .	1·06
Iron	1·05
2 bismuth 1 tin, and copper . . .	1·04
Copper and German silver	1·05
„ 32 bismuth 1 antimony . . .	1·00
„ bismuth	1·10
„ 32 bismuth 3 antimony . . .	1·29

Excepting the last, these quotients may be regarded as equal

The mean value, if the last number be excluded, = 1.06; and the deviations from the mean, which at the most amount to only 6 per cent., may be attributed to errors of observation. The last quotient may reasonably be looked upon as not comparable with the rest. The combination copper and 32 bismuth 3 antimony, in consequence of its great electromotive force, produces in the cylinders of the air-thermometer considerable variations of temperature on the reversal of the current; and therefore it is possible that the equations employed in the calculation of α , having been deduced on the hypothesis that the temperature-variations in the cylinders are only moderate, furnish for this combination a value of α which is not comparable with those for the rest of the combinations. This supposition is confirmed by the circumstance that the quotients appear to increase with the electromotive force. Hence we believe we are justified in assuming that *the ratio between the thermoelectric and the electromotive force of the alloys investigated is constant and equal to the ratio for the iron-copper and copper-bismuth combinations.*

Lastly, let us mention as another result of this investigation the remarkable constant diminution of the electromotive and thermoelectric forces which was observed in the bismuth-tin alloys. Of the pure metals, bismuth seems also to undergo such a change. Taking into consideration the known fact that the thermoelectrical condition of solid conductors is extremely sensitive to molecular changes (for example, slight impurities or a relative displacement of the molecules by mechanical treatment, as tension, torsion, pressure, &c.*), it seems very probable that the phenomenon in question is connected with some molecular alteration. By rapid solidification in the casting-mould free crystallization of the alloy is prevented, and a state of tension is produced, from which the molecules strive to free themselves. Simultaneously, chemical changes may take place; either the constituents of the alloy may enter partially into chemical combination with one another, or combinations that were formed in the casting may again fall asunder.

Similar molecular changes have been previously observed in some bodies. It is known, for instance, that the freezing-point of an ordinary thermometer rises a little in the course of time in consequence of the diminution in volume of its bulb. A similar phenomenon was observed by M. Edlund in 1857, when readjusting three gauged measures of the Swedish *Kanne*. The

* Thomson, Phil. Trans. 1856, p. 711. Magnus, Pogg. Ann. vol. lxxxviii. p. 469. Matthiessen found the thermoelectric force in bismuth and antimony very unequal, according as these metals were investigated after casting or in the state of compressed wires (Pogg. Ann. vol. ciii. p. 412).

volume of these measures, which were made of glass, was most accurately determined by F. Rudberg in the year 1833 at 100 cubic decimal inches; M. Edlund, however, found the volume of all three vessels perceptibly less, thereby proving a permanent structural alteration in the glass, which in twenty-four years had brought about a sensible lessening of the volume of these measures. Finally we will mention the transformation of the artificially produced monoclinic crystals of sulphur into an aggregate of minute rhombic crystals, the passing of arsenic acid from the amorphous state into the crystalline form, and other phenomena of this kind well known in chemistry.

It is probable that the bismuth-antimony alloys are subject to a change similar to that undergone by the bismuth-tin alloys, although the effect of it on the electromotive force may be too little to become apparent during the proportionally brief time of the present investigation. Whether all solid conductors are thus altered when subjected to mechanical treatment, and to what extent the alteration is accompanied by modifications of their other physical properties (as density, capability of conducting heat and electricity, &c.), may be brought to light by future inquiries.

Supplement.—I take leave here to mention that General Freiherr v. Wrede, of Stockholm, has recently discovered very remarkable alterations of the length of a rod, 1 decim. long, of the alloy 2 parts bismuth 1 part tin, as well as variations in its expansion by heat*. With respect to variations of the resistance to electric conduction of some alloys I made some experiments in Bonn at the end of last summer (1872). Through the kindness of Geheim-Rath Clausius I had the opportunity of using in these experiments an excellent universal galvanometer by Siemens. The results were:—The resistance of the alloy 2 parts bismuth 1 part tin diminished in 21 days 0·064 of its original quantity; that of the alloy 1 part bismuth 1 part tin, 0·07 in 9 days; and that of the alloy 1 part bismuth 2 parts tin, 0·06 in 4 days. The velocity of the change had its greatest value immediately after casting. The alloys 12 parts bismuth 1 part tin, 1 part bismuth 5 parts lead, and 10 parts bismuth 1 part antimony exhibited a pretty constant resistance to conduction, since the variations did not amount to more than 1 per cent.

* *Kongl. Svenska Vet.-Akad. Handl.* 1872.

VII. *On a simple Condensing Collector for Frictional Electrical Machines.* By SAMUEL ROBERTS, Esq.*

THE prime conductor of a frictional electrical machine is one armature of a condenser, the air being the dielectric, and the more or less conducting objects in the neighbourhood constituting the second armature. This obvious consideration suggests the form of a collector suitable for developing the full length of spark which the machine is capable of giving. Winter's ring aims at an enlargement of the conductor with as little dissipation as possible, and requires accurate and expensive workmanship. The exterior armature remains irregular and variable in the extreme; and we depend on the extent of the intervening dielectric for rendering the effect of these irregularities small. If we control the form of the exterior armature as well as that of the interior one, we really make an artificial condenser or Leyden battery, though the dielectric may still be air. I revert then to the old form of apparatus, in which, for medical purposes, the prime conductor was superseded by a Leyden jar; but as my object is not to accumulate a large quantity of electricity, but to produce long and frequent sparks, to obtain, in fact, the ordinary phenomena of a prime conductor armed with a ring and charged to a high potential, my Leyden jar is of minute dimensions; and in this consists what I conceive to be the novelty. The result of my experiments was so satisfactory that I venture to communicate it. The apparatus is very effective, of insignificant cost, and may be applied to any machine at once.

For a small machine with a cylinder 6 inches in diameter and 8 inches long, I take a glass tube $\frac{3}{10}$ inch in diameter and 13 inches long, hermetically sealed at one end. A fine copper wire, gauge 25, is inserted; and a piece of paper $2\frac{1}{2}$ inches broad is wrapped four or five times round the closed end. The open end with a little wire projecting is to be applied to the prime conductor; and the outer coating must be in good electric connexion with a metallic ball by means of a similar wire twisted once round the paper. I obtained in this way brilliant zigzag sparks $4\frac{1}{2}$ inches in clear length.

The charge of the tube at a given potential depends on the interior surface of the tube, and in part on the exterior surface, as well as on the wire and coating. Hence a tube of $\frac{3}{10}$ inch diameter and $\frac{1}{6}$ inch bore will give a spark of considerable volume, which may occasion a disagreeable shock. We can, however, modify the effect indefinitely; and this is a great advantage of the arrangement. The outer coating may be made

* Communicated by the Author.

movable, or can be expanded and contracted by simple sliding. The interior wire should not quite reach the end of the tube, because this is apt to be imperfectly annealed and easily cracks. Neither should the outer coating, if metallic, be tight and rigid; for I found that a tight brass slider, at once carrying the tube and forming the exterior coating, caused speedy rupture by spontaneous discharge. After several failures, the arrangement above described enabled the tube to bear a high charge without damage.

We may, however, with some advantage dismiss the conductor (which is commonly ill adapted to retain electricity at so high a potential), and substitute for it a long open tube mounted at one end with points and at the other with a brass knob, the points and knob communicating through the tube by a fine wire. I constructed a collector of this kind with a portion of a barometer-tube of $\frac{3}{10}$ inch diameter and $\frac{1}{12}$ inch bore, 19 inches long, and coated near the middle with tinfoil for 1 inch. This worked remarkably well, giving sparks 4 inches long; but a little additional length and a somewhat larger bore would be an improvement. For a large machine a piece of steam-gauge tubing would be suitable. The stem which carries the tube may be metallic, or of wood with a wire passing along it.

I observed that the full power of the machine was not yet exhibited, and therefore made a wooden point-holder supported by a glass stem. In the upper surface is an aperture in which, with a cork, a Leyden tube can be fixed in an upright position. A piece of wire twisted about the outer coating (which may be of paper rubbed with plumbago) communicates with a metallic knob. We may also fix the closed end of the tube in the aperture and connect the interior wire with the metallic ball. The free electricity on the covered surface of the tube is thus got rid of to some extent. This apparatus enabled me to obtain fine sparks in air between balls over $5\frac{1}{2}$ inches apart. The electricity, however, was frequently discharged over the glass of the cylinder; and it seemed that the limit allowed by the conditions of the insulation was nearly attained. I believe this result is remarkable for so small a machine.

The apparatus is so simple, compact, and effective that it should certainly be associated with every frictional electrical machine. Leyden jars are made use of in connexion with Holtz's machine with wonderful success; but the smallest I have seen are altogether too large for ordinary machines, and, as I have said, the pith of the matter is that the condenser must be charged and discharged frequently at the highest potential the machine can give. Of course it is desirable to varnish the tubes and otherwise take the usual precautions.

VIII. *On the Molecular Changes that accompany the Magnetization of Iron, Nickel, and Cobalt.* By W. F. BARRETT, Professor of Physics in the Royal College of Science, Dublin*.

THE magnetization of iron is accompanied by certain molecular changes in the metal which are well known to physicists. The object of this paper is an endeavour to extend our knowledge of some of these changes. Further inquiry in this direction seems to be needed as presenting one avenue of approach to a better insight of what may be termed the “molecular architecture” of a magnet.

The wonderful transition of iron from an ordinary to a magnetized condition makes no alteration in the appearance, the temperature†, the weight, or the total bulk of the iron; but it is associated with the changes alluded to, which are briefly as follows:—

1st. The act of magnetization causes a slight increase in the *length*, and a corresponding diminution of the breadth of an iron bar—a fact discovered by Mr. Joule in 1842, confirming the previous observations of MM. Gay-Lussac and Wertheim, that there was no alteration in the total volume of the iron. This elongation, however, does not occur when the iron is submitted to a definite longitudinal strain; and when the strain is still greater, the iron invariably *shortens* when magnetized.

2nd. A *sound* is emitted by the iron on magnetization and again on demagnetization. This was revealed by Mr. Page in 1837, and studied by many physicists subsequently. In iron wires the sound or clink seems composed of two distinct noises, one of which intensifies by a moderate strain, but is destroyed and the whole sound enfeebled by a still higher strain.

3rd. M. Wiedemann has proved that an iron wire hung in the centre of a helix and twisted is more or less *untwisted* when a current traverses the helix and magnetizes the wire. M. Matteucci has shown that twisting a magnet lessens its force, but stretching a magnet slightly adds to its power‡; and according to M. Guillemin, a strip of iron bent by its own weight is partly straightened by magnetization.

4th. The *conduction of heat* in magnetized iron is greater *across* than along the magnetic axis—a fact discovered by Dr. Maggi, and enlarged by Sir W. Thomson, who has shown that

* Communicated by the Author, having been read before the British Association at Bradford, September 1873.

† Nevertheless it is stated that repeated magnetization and demagnetization raises the temperature of an iron bar.

‡ A refined and capital method of exhibiting this is described by Mr. Gore in the ‘Proceedings of the Royal Society’ for January 28, 1869, vol. xvii. p. 265.

its precise analogue is to be found in the conduction of *electricity* in magnetized iron and nickel.

5th. A bar of wrought iron is more easily magnetized in the direction of its fibre; and steel once magnetized in a given direction and then demagnetized, is more readily magnetized in its first direction than in any other—a fact first pointed out by M. Marianni, and recently again observed by M. Jamin.

Lastly, it is well known that mechanical blows aid the assumption of magnetic power in steel, but tend to lessen and can even destroy it when assumed; and the same also is true of heat, which no doubt acts in a similar way, viz. by lessening the cohesion of the particles of steel.

All these facts may be embraced under the assumption first made by M. De la Rive, that magnetization is expressed by a definite movement, or a marshalling of the molecules of iron—the placing, as Dr. Tyndall puts it, of their longest dimensions end to end. Now iron is not the only magnetic body. Nickel and cobalt share the magnetic properties of iron to a very high degree; and to a much less extent the metals chromium and manganese are also magnetic. If, then, magnetization is an act associated with an altered structure of iron, we should expect to find a certain correspondence to iron in the properties of the other magnetic metals. That this is the case, is I think shown by the paper which has preceded this, “On the Relationship of the Magnetic Metals” *.

I was anxious to try further whether the molecular disturbances found on magnetizing iron were also exhibited by nickel and cobalt. One would of course expect to find analogous changes in these bodies; but I am unaware that they have hitherto been examined.

Messrs. Johnson and Matthey very kindly lent me an extremely fine bar of nickel and one of cobalt. Both bars are cylindrical, a little over 9 inches long and 1 inch in diameter. Though as pure specimens as they can be rendered commercially, the cobalt I find contains a very appreciable amount of iron; the removal of which body, as chemists well know, is a matter of the utmost difficulty.

The relative magnetic powers of these two bars deserves a moment's consideration. Nickel is invariably ranked above cobalt in the scale of magnetic metals, Faraday and others placing it next to soft iron. But the bar of nickel I have used, when submitted to the same magnetizing current as the cobalt bar, exhibits far less portative force than the cobalt. It is remarkable that the iron impurity contained in the cobalt is able to produce so powerful an influence. The nickel, like other speci-

* Phil. Mag. Dec. 1873.

mens I have met with, has very slight retentive power when magnetized, whereas the cobalt has a high degree of coercive force.

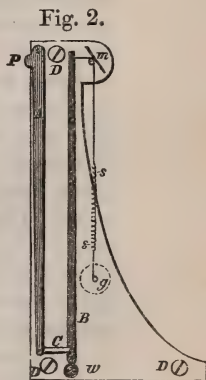
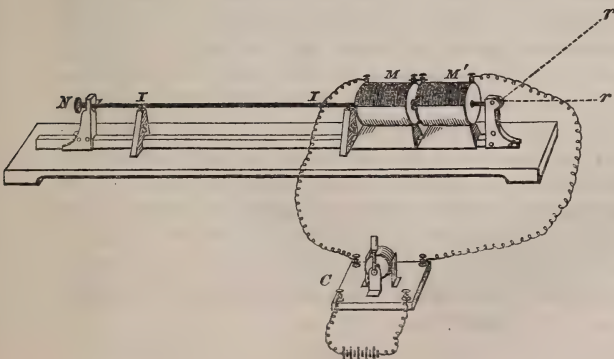
I.

Enclosing either of the bars within a helix of wire, a sound was emitted as soon as an interrupted current traversed the helix. The sound with *cobalt* was far the more powerful of the two, and was even more pronounced and more metallic than with a corresponding bar of iron. This fact, I believe, has not been noticed before. It is easy to obtain these sounds by merely using the coil of an electromagnet and drawing the terminal of the battery wire over a coarse file in a distant room.

II.

In order to examine whether the metals lengthened by magnetization, I had a special apparatus made for me by Messrs. Yeates, of Dublin and London. The instrument is a modification of the arrangement used by Dr. Tyndall, and described by him in his '*Researches on Diamagnetism*' (p. 240). Instead of being mounted vertically, the iron bar in my instrument is placed horizontally within the coils of a powerful electromagnet. One end of the bar is rigidly pressed by the end of a micrometer-screw, which is mounted on a sliding brass support that can be adjusted to any length that the bar under experiment may be. The other end of the bar presses against a system of levers, by which the least motion of the bar is largely multiplied. On an axle moved by the last lever a mirror is fixed; and upon this a beam of light is thrown, the reflected image being received on a distant scale. The whole is mounted on a firm mahogany base rather more than a metre long. The momentary warmth of a lighted match against the iron bar drives the reflected image through 2 or 3 feet of the scale.

The arrangement is shown in fig. 1. The soft iron bar, I I,



is mounted on the supports S, S, passing through the helices M, M', and abuts against the system of levers, which is shown enlarged in fig. 2. A micrometer-screw N drives the bar into close contact with the stud P (in fig. 2). The brass work supporting N slides along the woodwork, and can be firmly clamped to suit any length of the bar under experiment; the supports for the bar and for the helices also slide, so that the current can be sent round the bar at various positions. It was found necessary to employ wedges within M, M', to keep the bar from the lateral motion which occurred when the helices were animated. The connexions with the commutator C and the battery are evident. Fig. 2 shows the lever-frame with the front plate off. D, D are the screws of the pillars which hold the frame together. The first lever A is connected to the second B by the cross piece C; to the second lever is attached the counterpoise weight *w*, replacing the spring originally used. To the long arm of B is fastened a silk thread, which passes round a grooved wheel to the spiral brass spring *ss*; the tension of the spring can be regulated by turning the milled screw *g*; by this means the mirror *m* can be adjusted to any convenient position. The vertical scale on which is cast the reflected ray *r* (fig. 1) is not shown.

But the difficulties in the way of making determinations with this instrument were far more than I had even anticipated. The experience of the faults of one instrument led to the construction of another, which, though yielding better results, would at times perplex me by the most anomalous behaviour. On examination, these anomalies were traced to the hair spring which held the mirror in position; being of steel, it became magnetized by the current and was displaced accordingly. This was removed and a fine spiral copper spring substituted. The first results I obtained with this improved apparatus showed that the cobalt and iron behaved in a similar manner; each displaced the light in the direction of elongation 6·5 divisions of the scale on magnetization; but with the nickel no action whatever was observed. Increasing the battery power to twenty cells of Grove, a barely discernible movement took place in the direction of elongation. The very concordant results of two or three days' work seemed satisfactory; but unfortunately little reliance is to be placed in them, owing to the fact that I discovered at their close a serious source of error had been introduced through the inductive action on an iron pin fastening the levers, by which a disturbance was caused every time the current circulated round the helices. I have had lately the apparatus entirely remade, and hope to work at the subject again; meanwhile I am endeavouring to procure a purer specimen of cobalt*.

* I thought it possible a lecture demonstration of the elongation of

III.

I have now to allude to the deportment of iron when raised to a high temperature. Mr. Faraday has shown, in the last page of his 'Experimental Researches,' that a moderate degree of heat does not alter the magnetic capacity of iron, but diminishes the magnetic force of nickel and *increases* that of cobalt. At a greater elevation of temperature it is well known that nickel first loses its ordinary magnetic character, then iron, and finally cobalt. But Mr. Faraday has also stated (§§ 2343-2347) that though the magnetism of iron, nickel, and cobalt, as ordinarily exhibited, disappears at a high temperature, yet a feeble magnetic state remains, however exalted the temperature may be.

Some time ago several specimens of very tough fibrous iron were shown to me that had been obtained directly from cast iron by bringing high magnetic power to bear upon the latter metal when in a molten state. The process, which was patented, was thus popularly described by the 'Athenæum' for April 20, 1867:—"The experiment has been tried at one of the leading iron-works in Sheffield, and with complete success. The mode of operation, as roughly described, is to place a fixed electromagnet opposite an opening in the side of the furnace, to excite the magnet by means of a Smee's battery, so that the magnetism thus evoked may act upon the molten metal. The effect is surprising; the metal appears to bubble and boil, the melting is expedited, which economizes fuel; and the quality of iron is so much improved that for toughness and hardness it can hardly be equalled. It appears that some, if not all, of the impurities which remain after the ordinary process are eliminated by the use of magnetism." The scheme is so opposed to the ordinary views regarding the inertness of molten iron to magnetism, that any physicist must be naturally incredulous at this report, and would expect the patent to meet with the oblivion it has received. Nevertheless is it not possible, from Mr. Faraday's experiments, that some magnetic effect, not of translation but of direction, may be impressed on the molecules of molten iron? This is an inquiry to which I hope shortly to give more attention.

iron wire by magnetization might be made in the following manner:—Two soft iron wires were stretched on a monochord; round *one* wire, and nearly its whole length, a helix was coiled, leaving free space for the stretched wire to vibrate within. The wires were tuned to perfect unison; I then hoped that when the current animated the helix and so magnetized one wire, beats would be heard, owing to the diminished tension of the magnetized wire and, hence, unequal rate of vibration of the two. But I cannot say positively that such was the case. The experiment, however, was made under unfavourable circumstances, and needs repetition. With an optical representation of the combined vibration of the two wires a better effect might be anticipated.

The *resumption* of magnetic power by iron after being raised to bright incandescence is thus described by Mr. Faraday:—"The intensity of the force did not appear to increase until the temperature arrived near a certain point; and then, as the heat continued to diminish, the iron *rapidly*, but not instantaneously, acquired its high magnetic power, at which time it could not be kept from the magnet, but flew to it, bending the suspending wire and trembling as it were with magnetic energy as it adhered by one end to the core" (Exp. Res. § 2345).

Approximately at that temperature wherein a cooling iron wire resumes its magnetic state, a profound change occurs in the physical condition of the metal*. A momentary dilatation of the iron takes place; its thermoelectric position is reversed; a sound is emitted; and a sudden reheating, or "after-glow," is seen to diffuse itself throughout the metal just before it ceases to be incandescent; and its electric and thermal resistance at this point appears to undergo a change, though this has yet to be strictly determined.

IX. Notices respecting New Books.

Elementary Geometry. Books I., II., III., containing the subjects of Euclid's first four Books: following the Syllabus of Geometry prepared by the Geometrical Association. By J. M. WILSON, M.A., late Fellow of St. John's College, Cambridge, and Mathematical Master of Rugby School. Third Edition. London: Macmillan and Co. 1873. 12mo. pp. 188.

THE first two books only of the present work have been published before; they have been revised and in several respects improved. In the composition of the third book Mr. Wilson has been assisted by Mr. Moulton. Both these gentlemen, we believe, took part in drawing up the Syllabus of the Association for the Improvement of Geometrical Teaching, in conformity with which the present work has been written. It is needless to add that great care has been bestowed on the book. Its contents, so far as the text is concerned, are substantially those of Euclid's first four books. Many exercises and deductions are given in illustration of the text. In many cases the diagrams are so drawn as to show the data of the proposition. Playfair's treatment of parallel straight lines is given in place of the method used in former editions. Geometrical Loci are admitted into the text; and their use is illustrated by several examples. On the whole, the book, though by no means free from faults, is well executed; and a student who makes himself master of the text and is able to solve the problems and exercises will have a very good knowledge of the subject.

* The series of phenomena associated with this molecular change are detailed in my preliminary note on the subject in last month's *Philosophical Magazine*.

An author who takes in hand to write a book on the Elements of Geometry labours, however, under peculiar disadvantages. His work will be compared with a high standard; and mistakes which might have passed without notice in other subjects will be challenged in this. Besides, as there are strong—we do not say conclusive—reasons against the substitution of a new work for the recognized text-book, it will be expected that the author shall produce something substantially different and distinctly better. A writer who attempts to produce a synthetical Compendium of the Elements of Geometry substantially different from and distinctly better than the first four books of Euclid, undertakes a task of no ordinary difficulty; and, if we may judge from the preface to the third edition, Mr. Wilson is more aware of this than he was a few years ago. To intersperse the text with remarks and to add deductions are nothing to the point—notes and deductions to Euclid are already in existence. Nor is it much more to the point to make minor changes. Very many of Mr. Wilson's changes fall under the latter head. For instance:—the treatment of Parallels by Playfair's method; the proofs of Euclid, I. 8, I. 24, III. 26, 27, 28, 29 (Wilson, pp. 28, 29; 27; 112–120), and others; the grouping of the propositions; the separation of Problems from Theorems &c. It may be questioned whether in this there is on the whole improvement; at all events it is improvement of no great importance. To set against this there is a great deal which seems to us quite the reverse of improvement; and though we cannot exhaust the subject, we will go into details on three points.

(1) Mr. Wilson makes two classes of axioms—general and geometrical. Now, of course, it is true that such propositions as “if equals be added to equals the wholes are equal” are applicable to other than geometrical magnitudes; but then the application presupposes notions which the student of geometry may or may not possess. It may be true that when equal quantities of heat are added to equal quantities of heat the wholes are equal; but the student of geometry has nothing to do with this; he may not be able to understand what is meant by equal quantities of heat; and accordingly to him the axiom means that if equal angles or lines or areas are added to equal angles or lines or areas, the wholes are equal. The axiom, in fact, is to be construed with regard to geometrical matter. The like is true of the other so-called general axioms—at least as they stand in Euclid; and consequently there is nothing gained by the division. When we go further, however, and see what Mr. Wilson includes in the list of General Axioms, we cannot help suspecting the existence of some confusion of thought. Here are two of the general axioms:—“(3) If equals be added to equals the sums are equal.” “(7) If it is known that ‘If A is B then C is D,’ it follows that ‘If C is not D then A is not B.’” Now the former of these, as above remarked, relates to the *matter* of geometry; and where necessary Euclid quotes it amongst the premises of his reasoning, just as he would any other antecedent

theorem. The latter is merely a logical rule, and relates to the *form* of the reasoning. The following remark will put in a clear light the impropriety of classing (3) and (7) together under the same head. If it is right to rank as seventh amongst the axioms of geometry the rule that "a conditional proposition may be converted by negation," then the rule that "a universal affirmative proposition can only be converted by limitation" ought to be admitted into the list; and in fact it would be hard to say what rule of deductive logic could well be omitted*.

It is one thing to make here and there a remark on the form of the reasoning employed in proving this or that proposition, and another to erect a rule of logic into an axiom of geometry: the latter confuses things that differ; the propriety of making the former is a matter of opinion. Considering, however, that elementary geometry should be taught to rather young boys, we are strongly inclined to think it best to keep the rules of logic out of sight, or at least that they should be noticed, if at all, in the oral instruction of the teacher, and should not form a substantial part of the text-book. It must be remembered that when we are concerned with given matter we reason correctly, or can be shown to have reasoned incorrectly without explicit reference to the rules of logic. In a book of geometry, therefore, logical rules are extraneous matter, and as such are nearly certain to be treated inadequately, if admitted at all. Mr. Wilson illustrates this remark by devoting a page and a half to the conversion of propositions (pp. 3, 4); but he notices only conditional propositions, and his reason for doing so is that "if A is B then C will be D is the type of a theorem," although his own enunciations often take a different form (*e. g.* Theorems I., VI., X., &c.).

Mr. Wilson has four enunciations under the head of *Geometrical Axioms*. Two of them run thus:—“(2) Two straight lines which have two points in common lie wholly in the same straight line. (3) A finite straight line has one and only one point of bisection” (p. 9). The former of these is a true axiom; the latter admits of proof, and is therefore a theorem assumed to be true merely as a matter of convenience. Suppose Euclid had assumed the first eight and had begun his reasoning with the ninth proposition; his subsequent reasoning would have been unaffected; but surely it would have been an abuse of language if he had classed together as axioms “Two straight lines cannot inclose a space,” and “The angles at the base of an isosceles triangle are equal.” Yet this is just the sort of thing which Mr. Wilson has done.

(2) On coming to the *definitions*, it may be well to notice that in definition we have to do with nothing but the meaning of words. It follows that there must be some words which are incapable of definition, and whose meanings must be arrived at by a direct appeal to the intuitions of sense—to experience, in fact. There may be good reasons for saying that a line is “length without breadth;” but it is hardly a definition, as the words contained in it (“length”

* Mr. Wilson's sixth axiom also is purely logical.

and "breadth") cannot be understood without a previous knowledge of the meaning of the word line. Compare the definition of a line with that of a square—"a four-sided figure which has its sides equal and its angles right angles;" and the difference is at once apparent: in the former the definition is performed by means of words of an equal order of difficulty, in the latter by means of an analysis of the name into others simpler than itself. Mr. Wilson is plainly not contented with Euclid's definition; but he does not mend matters by giving the definition of a line thus:—"A line has position but neither breadth nor thickness." The fact is that the difficulty cannot be got over. There are many words of which we all know the meaning, but which we cannot define: such words as red or blue are instances; and so are a point, a line, a superficies, an angle. Mr. Wilson's account of a straight line is quite a curious instance of how nearly a writer may get to a distinction and yet miss it. He tells us:—"Def. 5. A *straight line* is such that any one part must, however placed, lie wholly on any other part if its extremities are made to fall on that other part." This is plainly a property of a straight line, or rather of two straight lines, not a definition; it is in fact the second geometrical axiom with its wording slightly altered. If a definition is to be attempted at all, it would be hard to produce a better than the old one—"A straight line is that which lies evenly between its extreme points;" but, of course, the word *evenly* as much requires definition as the word *straight*. Mr. Wilson adds to his definition the remark, "A stretched string suggests the notion of *straightness*, which is in fact incapable of elucidation" (p. 6). If he had said that "a string stretched between two points suggests the notion of a straight line, a term which is in fact incapable of definition," we should have entirely agreed with him; but as to the notion of a straight line (or straightness) being incapable of *elucidation*, that is quite another point. The notion is elucidated in Whewell's 'Philosophy of the Inductive Sciences,' vol. i. pp. 96, 97, as Mr. Wilson will find on reference.

(3) We have hitherto dwelt only on axioms and definitions. We will now advert to a fault which pervades the whole book, though want of space compels us to consider it with regard to only a single instance. The fault is impatience of the restrictions of Elementary Geometry; the instance is the treatment of angles. Euclid, as is well known, does not recognize any angle which might not be an angle of a triangle. There seems good reason for this: the notion of an angle that the learner brings with him to the subject is that of a knee or corner; to make him thoroughly familiar with the notion and able to reason upon it correctly is enough in the first instance, and the more as the notion is fully sufficient for all the requirements of the first four books. Mr. Wilson thinks otherwise; and so to angles right, acute, and obtuse he adds the "straight angle" [one of two right angles] and the "reflex angle" [one greater than two right angles]. Now, in the *first place*, he uses no term to denote merely an angle less than a "straight angle;" consequently he always terms such an angle simply *an angle*; in other

words, throughout the greater part of the book Euclid's restriction is tacitly reimposed, and this even in cases where, if the consideration of reflex angles is to be thought advisable at all, we should have expected to see them noticed. *E. g.* Cor. p. 49 [Euclid I. 32, Cor. 2] is expressly limited to any convex polygon; Ex. 2, p. 39, Ex. 5, p. 40 are not always true if the quadrilateral has a reentering angle. We raise no objection to this but want of consistency. *Secondly*, with regard to the "reflex angle," it is merely said to be greater than two right angles. Now take any Euclidian angle and denote it by θ ; are we to understand by the corresponding reflex angle $2\pi - \theta$ or $2n\pi \pm \theta$? If the former, here is a restriction much more arbitrary than Euclid's; if the latter, we have a conception proposed to a beginner which is well known to be one presenting great difficulty to those who are beginning Trigonometry and have therefore made some progress in Mathematics. Mr. Wilson has an "axiom" which asserts that "an angle has one and only one bisector;" but upon these terms here is an angle A O B which has one, or one of two, or two bisectors, according as A O B means some one, or any one, or all of the series $2n\pi \pm \theta$. *Thirdly*, with regard to the "straight angle." A boy comes to his teacher with the notion that an angle is something resembling a corner of a square or triangle; and without delay his teacher introduces him to a notion of this kind:—Draw a straight line A B and take in it any point C; at C there are two straight angles, one on each side of the line—indeed as many as we please, for there is no occasion to restrict ourselves to a single plane. He comes with a notion that an angle is something between two straight lines that meet at a point, and he is taught without delay that he has only to draw a line an inch long and put a dot upon it to produce an infinite number of angles. This may be all very well for the teacher; but what is it for the boy? Not only, however, is it of more than doubtful expediency to put the notion of a straight angle before a beginner, but Mr. Wilson does it in a way which looks as if he had hardly considered all the consequences that ought to follow from its introduction. We will try to explain what we mean. It might perhaps be contended that the theorem "all straight angles are equal" needs distinct proof; though it is not easy to see what the theorem would add to the axiom, "two straight lines, which have two points in common, lie wholly in the same straight line." Now Mr. Wilson does not deem it necessary to give a formal treatment of this theorem; but he does deem it necessary to do what is much more surprising. He gives Theor. I. "All right angles are equal to one another." Theor. II. "If a straight line stands upon another straight line it makes the adjacent angles together equal to two right angles." Theor. III. "If the adjacent angles made by one straight line with two others are together equal to two right angles, these two straight lines are in one straight line." From Euclid's point of view this would be very proper; but from Mr. Wilson's point of view a right angle is only half a straight angle. Now let us try the effect of writing in these theorems, half a straight angle for right angle, and straight

angle two right angles. We then see that the object of Theorem I. is to prove that the halves of straight angles are equal, *i. e.* it is a particular case of Axiom 5. Mr. Wilson might just as well have a theorem to prove that the halves of equal straight lines must be equal. The object of Theorem II. is to prove that if a straight line divides a straight angle into two parts, the two parts are together equal to a straight angle. With regard to Theorem III., we remark that a straight angle is that "made by a straight line with its continuation;" so that the point to be proved is that a straight line and its continuation are in one straight line. In short, from Mr. Wilson's point of view, these "Theorems" are not theorems at all; and he has failed to see it.

X. *Proceedings of Learned Societies.*

ROYAL SOCIETY.

[Continued from vol. xlv. p. 410.]

April 3, 1873.—William Sharpey, M.D., Vice-President, in the Chair.

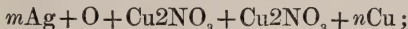
THE following communication was read:—

"On an Air-Battery." By J. H. Gladstone, Ph.D., F.R.S., and Alfred Tribe, F.C.S.

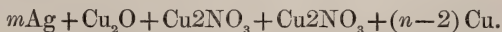
The galvanic battery which we are about to describe is founded on a reaction that we brought under the notice of the Royal Society last spring*. We then showed that if pieces of copper and silver in contact are immersed in a solution of nitrate of copper in the presence of oxygen, a decomposition of the salt ensues, with the formation of cuprous oxide on the silver and a corresponding solution of the copper, while a galvanic current passes through the liquid from copper to silver. We stated, moreover, that this was no isolated phenomenon, but only one of a large class of similar reactions. It seemed desirable to examine more fully the history and the capabilities of the electrical power thus produced.

It was previously ascertained that the combination of the oxygen takes place only in the neighbourhood of the silver; and the following formulæ may serve to render the chemical change and transference more intelligible:—

Before contact,



after contact,



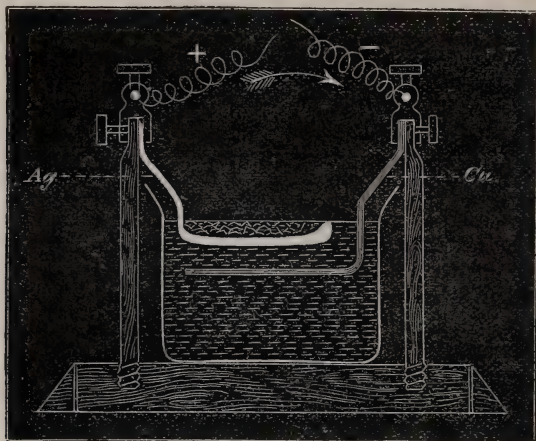
This action is evidently a continuous one until either the oxygen or the copper fails.

Now the oxygen of the atmosphere is practically unlimited in

* Proc. Roy. Soc., April 1872, vol. xx. p. 290.

amount ; but there is a difficulty in bringing any large quantity of it into contact at once with the silver and the dissolved salt.

To facilitate this, we arrange that the silver plate should have a horizontal position just under the surface of the liquid in the cell ; and, in fact, we convert it into a small silver tray full of crystals of the same metal which rise in projections to the very surface. The copper plate lies horizontally under it, separated, if need be, by a piece of muslin ; and connexion is made by a wire as usual. The vertical part of the copper plate, from a little above the liquid to the bend, should be varnished ; otherwise solution principally takes place there, which causes the horizontal part of the plate to drop off. Holes are made in the silver tray with the view of shortening the communication between the air-surface and the copper plate and of facilitating the movements of the salt in solution. The solution itself may be contained in a shallow trough or saucer, and the whole arrangement will be somewhat as in the sectional view here given :—



That dissolved oxygen is absolutely necessary for this chemical change has been already shown ; but it was interesting to measure by a galvanometer the difference of the currents obtained by means of an ordinary, that is aerated, solution of copper nitrate, and one from which the air had been separated to the greatest possible extent. A Thomson's galvanometer was employed, which had a resistance of 2631.5 units at 18° 3 C. Two cells were prepared with vertical plates and alike in all respects, except that the one contained an ordinary 6 per cent. solution of copper nitrate, and the other a similar solution which had been deoxygenized by the means described in our former paper. Another experiment was made with a different pair of cells and an 11 per cent. solution. It was necessary to use the 1-99 shunt ; and the following were the amounts of deflection :—

Time after immersion.	Expt. I.		Expt. II.	
	Oxygenized.	Deoxygenized.	Oxygenized.	Deoxygenized.
1 minute.	78	14	130	11
4 minutes.	72	9	90	8
12 "	68	6	75	6
49 "	—	—	58	3·5

The contrast is evident. That the deoxygenized solution does give a deflection at all is due partly to the difficulty of excluding air, and partly, perhaps, at first to the oxygen condensed on the surface of the silver plate. The effect due to the water itself is inappreciable.

From the nature of the reaction it might be expected that the current would gradually diminish on account of the using up of the dissolved oxygen in the neighbourhood of the silver; such a diminution always does take place, at least after the first few vibrations of the needle.

It might be expected, too, that when the amount of action has run down considerably, the mere moving of the liquid so as to bring fresh parts of the solution against the silver would augment the currents. It does so.

The same might be predicted from stirring up the crystals of silver in the tray so as to expose new surfaces. This also was found to be the case.

And, again, it might be anticipated that if the wire were disconnected for a time so as to allow the oxygen to diffuse itself from other parts of the solution, and the connexion were made, the current would be found as strong, or nearly so, as before. That also is true in fact.

A cell with the plates connected by a wire was placed under a bell-jar full of air over mercury. The mercury gradually rose inside, as might be expected from the absorption of the oxygen in the air.

The necessity of oxygen and the avidity with which it is taken up are both illustrated by the following experiment:—Two cells with horizontal plates were prepared alike in every respect, except that the first was filled with a solution simply deprived of oxygen, the second with a solution through which a stream of carbonic-acid gas had been passed for some time. The first was placed in the air, the second in a vessel from which the air had been expelled by allowing carbonic-acid gas to flow into it for an hour or two.

The deflections obtained were as follows, the 1-999 shunt being used and the temperature being 13°·7 C:—

Time after immersion.	In air.	In CO ₂ .
1 minute.	165	76
5 minutes.	135	62
10 „	135	58

As the cell in an atmosphere of carbonic-acid gas showed considerable action, in fact nearly half as much as that in the air, each cell was short-circuited for 23 hours, with the expectation that any oxygen in the closed vessel would be used up; and, indeed, the most prominent crystals of silver in the cell in carbonic-acid gas became reddened, while a cuprous deposit extended over the whole of the crystals in the other cell. When, however, the short wires were removed and the galvanometer interposed, the cell in the air gave a deflection of 136, practically the same as before, but that in carbonic-acid gas, instead of showing a great decrease, rose to 80. It was then found that the vessel containing the latter slowly admitted air; so the contents were swept out by a fresh stream of carbonic-acid gas, and it was made properly air-tight. After connexion by a short wire for 3 days the galvanometer indicated a deflection of 20, that of the cell in the air being 110, temperature 10° C. As this showed a very great reduction of the chemical action, carbonic-acid gas was again passed through the vessel for an hour or two; and after a connexion of two more days the indication of the galvanometer was only 3, while the other cell gave 115, the temperature being now 10°·5 C. The action, therefore, was at last reduced almost to nothing; and the original fault in the experiment brought out, perhaps more clearly than would otherwise be seen, how eagerly the solution will absorb even minute quantities of oxygen from the surrounding gas.

An important point to determine was the best strength of the copper nitrate solution. Six per cent. was generally preferred, for two reasons:—first, it gives about the maximum of effect—a solution four times as strong gives less than half the deflection, and a solution only a quarter as strong gives only two thirds; secondly, a stronger solution than this 6 per cent. is apt to produce a deposit, not of pure cuprous oxide, but of a subnitrate, which was supposed to clog up the silver crystals to a greater extent.

Another point investigated was the best proportion between the areas of the metallic surfaces. Experiments were made with vertical plates, in which the silver was kept at a uniform size and the copper was diminished by covering it more and more with varnish; and another set was made in which the copper remained the same, while the silver plate was reduced.

The results may be thus exhibited:—

Proportion of surfaces.		Deflection.		
Silver.	Copper.	Expt. I.	Expt. II.	Expt. III.
1	0.25	24	23	..
1	0.5	28	27	..
1	0.75	31	30	..
1	1.0	33	32	28
1	1.33	..	..	28
1	2.0	..	..	32
1	4.0	..	..	30

The increase of the copper surface, therefore, has comparatively little effect.

Proportion of surfaces.		Deflection.		
Copper.	Silver.	Expt. I.	Expt. II.	Expt. III.
1	0.25	..	..	7.5
1	0.5	..	..	16
1	0.75	..	..	21
1	1.0	33	32	28
1	1.33	41	40	..
1	2.0	56	54	..
1	4.0	96	92	..

The increase, therefore, of the silver or negative metal causes an almost proportionate increase in the chemical action. This, doubtless, arises from the necessity of oxygen, and explains the value of the large surface exposed by the silver crystals in the tray.

The effect of heat on the action of this cell was examined; it increases the action greatly: thus an arrangement which gave a deflection of 40 at 20° C. gave one of 250 at 50° C.; and the augmentation was observed to be much more rapid in the higher than in the lower portions of this range of temperature.

If the formula given above for the reaction be a true one, it follows that every atom of copper deposited on the silver in the state of suboxide must be compensated by an atom of copper dissolved from the copper plate. This was proved quantitatively. In a cell that had been in action for a week the loss of the copper plate was 0.391 grm., while the suboxide deposited on the silver was found to be equivalent to 0.398 grm. of metallic copper. This deposit of suboxide, though it soon forms apparently a complete

covering to the silver, does not greatly diminish the action; it is probably porous, besides being itself a conductor of electricity. In some cases we have found it deposited in crystals sufficiently large to be seen by the naked eye, and which are shown by a magnifying-glass to be regular octahedra.

The internal resistance of this battery, as might be expected, is small.

The electrolytic power of the current was examined. One cell, the plates of which were about two inches in diameter, was found sufficient to decompose such metallic salts as the nitrates of copper, silver, or lead, copper sulphate or stannous chloride, in aqueous solution, when platinum was used for the negative electrode, and for the positive the same metal as existed in the salt experimented on. Six cells were sufficient to decompose dilute sulphuric acid slowly and dilute hydrochloric acid pretty quickly, copper electrodes being employed.

The theoretical interest of this battery lies mainly in the fact that it differs essentially from every other galvanic arrangement, inasmuch as the binary compound in solution is incapable of being decomposed either by the positive metal alone or by the two metals in conjunction; it cannot serve, in fact, as the liquid element of the circuit without the presence of another body ready to combine with one of its constituents when set free.

Grove's gas-battery is essentially different from ours if the oxygen and hydrogen condensed on the platinum plates play the part of the two metals; but it closely resembles ours if hydrogen acts the part of the positive and platinum that of the negative metal; the dilute sulphuric acid, a hydrogen compound, will then be decomposed on account of the simultaneous presence of the oxygen, which can combine with the liberated hydrogen. Viewed in this manner Grove's gas-battery is only a special case of the general reaction which we have described in our previous paper; and the formula will be:—

Before contact,



after contact,



The practical interest of our arrangement lies in the fact that it is an approximation towards a constant air-battery. Should it ever come into use elsewhere than on the lecture-table, it will probably be in the form of a combination of zinc and copper, with an aerated solution of zinc chloride; for that arrangement has an electromotive force six times that of the arrangement we have more particularly studied, and about three quarters that of a Daniell's cell. The numbers representing the difference of potential between the two metals, which were actually obtained by means of an electrometer belonging to Sir William Thomson, were:—

Silver and copper with deoxygenized copper nitrate	4
Ditto „ oxygenized ditto	8 to 11
Copper and zinc with chloride of zinc	62
Ditto „ water	68
Ditto „ Daniell's cell	83

Chloride of zinc is preferred to the sulphate, as it offers less internal resistance, and a solution of 20 per cent. is recommended as about the best conductor*. A single cell of this description is capable of decomposing dilute sulphuric or hydrochloric acid when copper electrodes are employed. The two metals might be arranged as in a Daniell's battery; the zinc would of course require no amalgamation; and the whole might be left for weeks or months without any attention. The oxide of zinc produced generally falls to the bottom of the vessel, and may be separated whenever it is thought desirable.

The power is thus obtained at a minimum of expense; for the oxygen which combines with the zinc costs nothing. Such a battery would appear to be specially adapted to cases where the galvanic current has to be frequently broken, as in telegraphy; for at each period of rest it renews its strength by the absorption or diffusion of more oxygen from the air.

May 1, 1873.—William Spottiswoode, M.A., Treasurer and Vice-President, in the Chair.

The following communication was read:—

“On the Condensation of a Mixture of Air and Steam upon Cold Surfaces.” By Osborne Reynolds, M.A., Fellow of Queen's College, Cambridge.

1. The object of this investigation is to ascertain how far the presence of a small quantity of air affects the power of a cold surface to condense steam. *A priori* it seemed probable that it might retard condensation very much; for when pure steam comes up to a cold surface and is condensed, it leaves an empty space which is immediately filled with fresh steam; so that the passage of the steam up to the cold surface is unobstructed, and if the surface could carry off the heat fast enough, then the rate of condensation would be unlimited. If, however, the steam is mixed with air, then, as the mixture comes into contact with the cold surface, the steam will be condensed and the air will be left between the fresh steam and the cold surface; so that after condensation has commenced that surface will be protected by a stratum of air, and fresh steam will have either to displace this or pass through it before it in turn can be condensed.

2. This question, besides its philosophical interest, has important practical bearings on the steam-engine.

First. If the quantity of air mixed with the steam affects the rate at which it condenses, then the ratio which the pressure of air bears to the pressure of steam in a condenser will materially affect

* On the authority of Mr. Herbert MacLeod.

its efficiency: this is particularly important with reference to the *surface-condenser*.

Second. If air prevents the condensation of steam, then by sending air into the boiler of a *high-pressure engine*, the condensation at the surface of the cylinder will be prevented, which, if allowed to occur, becomes a source of great waste; for when the steam comes into a cold cylinder it condenses, heating the cylinder and leaving water, which will again be evaporated as soon as the steam escapes; and this, in evaporating, will cool the cylinder. By preventing this, the mixing of air with the steam would effect the same object as the steam-jacket, only in a more efficient manner; for the heat communicated to the steam in the cylinder from the jacket is not nearly so effective as that which is communicated from the boiler, in consequence of the steam in the cylinder being at a lower temperature than that in the boiler.

3. The experiments for this investigation were, by the kind permission of Dr. Roscoe, carried out by Mr. Pasley, a student in the Chemical Laboratory of the Owens College; and I beg to tender him my best thanks.

4. In making these experiments two objects were particularly kept in view:—

First. To ascertain if there is a great difference in the rate of condensation of pure steam and a mixture of steam and air—to ascertain in fact whether pure steam condenses at an unlimited speed.

Second. To ascertain if (and according to what law) the effect of air on the condensation increases as the proportion of air to steam increases.

5. Of these two undertakings the first is much the most difficult. The rate of condensation of pure steam is so great that it is practically impossible to measure it; and to institute a comparison between this and the condensation of a mixture of steam and air is like comparing the infinite with the finite. It is practically impossible to keep any surface cold when an unlimited supply of pure steam is condensed upon it, so that under such circumstances the quantity of pure steam condensed is limited by the power of the surface to carry off the heat. The best method of obtaining a quantitative result seems to be by introducing sufficient cold water into a flask of steam to condense it all, and ascertain whether this condensation is effected suddenly or slowly.

6. The presence of hot water in the flask with the steam very much assists in ascertaining the rapidity of condensation. When there is no hot water in the flask, the condensation by the injected water is only a question of time; the gauge will come to the same point whether the condensation is quick or slow, the only difference being in the speed at which it will rise—a difference not easy to appreciate, especially when the motion is quick. But if hot water is present, then as the steam in the flask is condensed it is replaced by fresh steam from the water, and the interval between the condensation and the consequent ebullition is the only time

allowed for the creation of a vacuum ; the vacuum which is attained in the interval will therefore depend on the rapidity of condensation. The interval will be very short ; and the better the vacuum the shorter it will be ; so that unless the condensation is very sudden, there will be but a slight reduction of pressure.

If, however, the condensation is really instantaneous, a perfect vacuum may exist for an instant. Hence, when there is water in the flask, the rapidity of condensation is indicated by the height to which the gauge rises, instead of the speed with which it rises ; and this is much easier to estimate.

7. The apparatus employed in making these experiments consisted of a glass flask fitted with a mercurial vacuum-gauge and pipes for admitting water and air, or allowing steam to escape.

The flask and all the pipes were freed from air by boiling ; and when all the air had been driven out the pipes were closed, the lamp removed, and the flask allowed to cool until the gauge showed a slight vacuum ; the water-pipe was then opened and a few drops of water allowed to enter and fall through the flask ; as they did so the mercury rushed up the gauge and, by its momentum, above the point for a perfect vacuum, showing that the condensation was instantaneous. Immediately afterwards the gauge fell nearly to its starting-point. Next, the flask was allowed to cool and a little air was let in (about equal to half an inch of mercury in the gauge, or about a sixtieth of the volume of the flask). The lamp was then replaced, and the operation was repeated as before ; this time, however, as the cold water entered the mercury did not rush up the gauge, but rose slowly a small distance and there remained.

8. This experiment shows, therefore, that there is a great difference in the rates at which pure steam and steam with air condense on a cold surface, so great in fact that the speed with pure steam must be regarded as nearly infinite.

9. To compare the various effects of different quantities of air, two methods have been used, which may be described as follows :—

I. A surface-condenser is formed within the boiler or flask, so that the steam may be condensed as fast as it is generated. Then, when a flame of a certain size acts on the boiler, the effect of the air is to cause the pressure of steam in the flask to increase. This method is founded on the assumption that the rate at which steam will condense at a cold surface is, *cæteris paribus*, proportional to its pressure—an assumption which is probably not far from the truth.

II. With the same apparatus as in method I. the rate of condensation is measured by the quantity of water condensed in a given time, obtained by counting the drops from the condenser, the pressure within the flask being kept constant. This method does not involve any assumption ; but the conditions for its being accurate are such as cannot be obtained ; for not only must the temperature of the condenser and the temperature of the steam remain constant, but the pressure of the steam must also remain constant, and if the two former conditions are fulfilled the latter cannot be ; for the temperature of the steam will be the boiling-point of the water in

the flask; and if this is to remain constant, the pressure of air and steam must be constant, and therefore, as the pressure of the air increases, the pressure of the steam must decrease. This variation of pressure is not very great; and its effect may be allowed for on the assumption that the condensation is proportional to the pressure of steam. This is accomplished by dividing the drops by the pressure of the steam.

These methods, neither of which, as it appears, is rigorous, seem nevertheless to be the best; and fortunately the law which the effect of the additions of air follows is of such a decided character as to be easily distinguished; and the two methods give results which are sufficiently concordant for practical purposes.

10. The apparatus employed in these experiments consisted of a glass flask, in which a surface-condenser was formed of a copper pipe passing in and out through the cork. This pipe was kept cool by a stream of water, and was so fixed that all the condensed water dropped from it, and the drops could be counted. The flask was freed from air by boiling; the volume of air passed into the flask could be accurately measured; and ample time was allowed for the air in the flask to produce its effect before more was admitted.

For the experiments according to method I., the flame under the flask and the stream of water through the condenser were kept constant from first to last. For those made according to method II., in one case the stream of water was kept constant, and in the other it was altered, so that the effluent water was kept at a constant temperature.

11. The results of these experiments are shown in Tables I., II., III.

The letters which head the columns have the following meanings:—
f stands for the volume of the flask in cubic centimetres.

a stands for the volume of the air at the pressure of the atmosphere.

*h*₀ stands for the height of the barometer in millimetres of mercury at the time of the experiment.

*h*₁ stands for the height of mercury in the gauge in millims.

*t*₀ stands for the temperature Centigrade of the effluent water.

*t*₁ stands for the temperature of the water in the flask, found from Regnault's tables of boiling-points.

*p*₁ = *h*₀ - *h*₁ stands for the pressure within the flask in millims. of mercury.

$p_2 = \frac{a}{f} + h_0 \frac{t_1 + 274}{t_0 + 274}$ stands for the pressure of the air within the flask corrected to the temperature *T*₁.

*p*₃ = *p*₁ - *p*₂ stands for the pressure of the steam.

$\frac{p_2}{p_3}$ stands for the ratio of the pressure of the air in the flask to that of the steam.

TABLE I.

$$h_0=756, \quad t_0=9, \quad f=500.$$

$a.$	$t_1.$	$h_1.$	Drops per minute.	$p_1.$	$p_2.$	$p_3.$	$\frac{1}{p_3}.$	$\frac{p_2}{p_3}.$	$\frac{2000}{p_3}.$
0	9	754	...	2	0	2		0	...
0	22	736	...	20	0	20	0500	0	100
1.5	36	712	...	44	2.4	41	0240	06	48
5.0	52	654	...	106	8.7	97	0110	09	22
10	66	557	56	199	18.4	183	0055	10	11
13	70	521	...	235	23.7	211	0050	11	10
21	77	433	...	323	40.0	283	0035	14	7
30	84	330	...	426	57	368	0027	15	5.4
40	88	264	...	492	77	414	0024	18	4.8
50	93	179	56	577	97	479	0020	20	4
60	96	115	...	641	117	523	0019	22	3.8
70	98	55	...	701	138	562	0017	24	3.4
80	100	0	...	756	159	596	0016	26	3.2

TABLE II.

$$h_0=457, \quad f_0=500.$$

$a.$	$t_0.$	$t_1.$	$h_1.$	Drops per minute.	$p_1.$	$p_2.$	$p_3.$	$\frac{p_2}{p_3}.$	$\frac{\text{Drops}}{p_3}.$	$\frac{\text{Drops}}{p_3}.$
0	27	66	567	100	190	0	190		53	42.4
2.5	24	"	572	84	185	4.5	180	022	45	36
5	20	"	582	59	175	9	166	055	35	27
10	13	"	582	21	175	19	156	12	14	11.2
27	10	"	582	10	175	48	127	39	76	6.0
37	10	"	579	10	177	66	111	66	9	7.2
50	9	"	572	8	184	90	94	10	8	6.4

TABLE III.

$$h_0=748, \quad t_0=11, \quad f=500.$$

$a.$	$t_1.$	$h_1.$	Drops per minute.	$p_1.$	$p_2.$	$p_3.$	$\frac{p_2}{p_3}.$	$\frac{\text{Drops}}{p_3}.$
0	6	741	..	7	0	7	0	0
...	47	663	106	85	0	85	0	125
3.2	66	557	106	191	6	185	032	60
5	"	"	56	"	9	182	050	30
10	"	"	21	"	18	173	104	11
15	"	"	17	"	27	164	163	10
20	"	"	12	"	36	155	23	8
30	"	552	10	196	54	142	39	7
40	"	557	8	191	72	119	60	6 $\frac{1}{2}$
50	"	562	7	186	90	96	93	7

12. Table I. shows the result of an experiment after the first method, during which the flame and condensation remained constant, whilst the pressure within the flask increased with the quantity of air.

Table II. shows the result of an experiment after the second method, in which the pressure within the flask remained constant, whilst the flame and condensation were reduced as the air was admitted. In this experiment the rate at which the water passed through the condenser was constant from first to last, and consequently the temperature of the effluent water varied with the condensation.

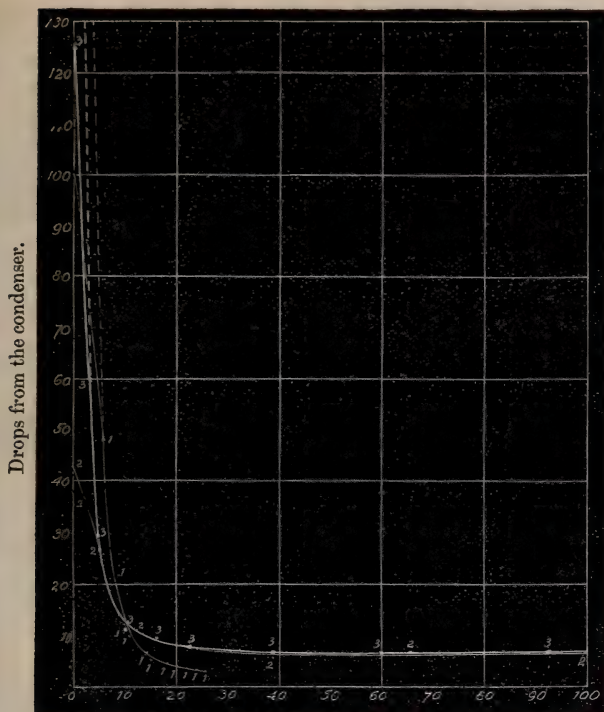
Table III. shows the result of an experiment, also made according to the second method, but in which the quantity of water flowing through the condenser was so varied that the temperature of the effluent water remained constant.

13. Each of these Tables shows the effect of air on the condensation in a very definite manner; but the results as given in the column p_3 in Table I. cannot be compared with the $\frac{\text{Drops}}{p_3}$ in Tables II. and III. as they stand; for these show the effect of the air in a series of increasing figures. If, however, these figures show the power of the air to diminish condensation, then they will be inversely proportional to the quantity of water condensed, *i. e.* what would have been condensed if the pressure and other things had remained constant. Hence the numbers in the column $\frac{1}{p_3}$ should be proportional to the numbers in the column $\frac{\text{Drops}}{p_3}$ in Tables II. and III.

In order to compare the results of these experiments, the results in each Table have been multiplied by a common factor, so that they may be the same when the pressure of air is one tenth that of the steam. Thus the numbers in the column $\frac{1}{p_3}$ in Table I. have been multiplied by 2000, and numbers under $\frac{\text{Drops}}{p_3}$ in Table II. by 7. The result of the experiments thus reduced are shown in the curves 1, 2, 3.

The point of no air might have been chosen as the point in which the curves should coincide; but, as has been previously explained, the results under such circumstances are to be taken as indicating the power of the condenser to carry off the heat. Had it been possible to keep the condenser cool, then there is reason to believe that there would have been no limit to the condensation of pure steam, and that the true form of the curves is like that shown by the dots.

Although the curves do not coincide, yet they are all of the same form, and the difference between them is not greater than can be accounted for by the disturbing causes already mentioned. They all show that the effect of air begins to fall off rapidly when its pressure amounts to one tenth that of the steam, and that when it amounts to about one fourth that of the steam the admission of more air produces scarcely any effect.



Ratio of the pressure of air to that of steam.

14. *Conclusions.*—The conclusions to be drawn from these experiments are as follows :—

1. That a small quantity of air in steam does very much retard its condensation upon a cold surface ; that, in fact, there is no limit to the rate at which pure steam will condense but the power of the surface to carry off the heat.

2. That the rate of condensation diminishes rapidly and nearly uniformly as the pressure of air increases from two to ten per cent. that of the steam, and then less and less rapidly until thirty per cent. is reached, after which the rate of condensation remains nearly constant.

3. That in consequence of this effect of air the necessary size of a surface-condenser for a steam-engine increases very rapidly with the quantity of air allowed to be present within it.

4. That by mixing air with the steam before it is used, the condensation at the surface of a cylinder may be greatly diminished, and consequently the efficiency of the engine increased.

5. That the maximum effect, or nearly so, will be obtained when the pressure of the air is one tenth that of the steam, or when about two cubic feet of air at the pressure of the atmosphere and the temperature 60° F. are mixed with each pound of steam.

15. *Remarks.* As this investigation was nearly completed my

attention was called to a statement by Sir W. Armstrong, to the effect that Mr. Siemens had suggested as an explanation of the otherwise anomalous advantage of forcing air into the boiler of a steam-engine, that the air may prevent, in a great measure, the condensation at the surface of the cylinder. It would thus seem that Mr. Siemens has already suggested the probability of the fact which is proved in this investigation. I am not aware, however, that any previous experiments have been made on the subject, and therefore I offer these results as independent testimony of the correctness of Mr. Siemens's views as well as of my own.

GEOLOGICAL SOCIETY.

[Continued from vol. xlv. p. 329.]

February 5, 1873.—Warrington W. Smyth, Esq., F.R.S., Vice-President, in the Chair.

The following communication was read:—

1. "On the Oolites of Northamptonshire.—Part II." By Samuel Sharp, Esq., F.S.A., F.G.S.

In the first part of this memoir the succession of beds in the neighbourhood of Northampton was shown to be as follows:—

	Clay	} Great Oolite.	
	White Limestone.....		
	Clay with Ferruginous Band		
	(“ Upper Estuarine”)		
	Line of Unconformity.		
Northampton	{ Sand with Plant Bed	} Inferior Oolite.	
	{ (“ Lower Estuarine”).....		
	{ Variable Beds		
	{ Ironstone Beds.....		
Sand	Upper Lias Clay.		

The Great Oolite limestone of this section has been confounded, even up to the present time, with a limestone (frequently Oolitic) which occurs between Kettering and Stamford, is prevalent about the latter town, extends through Rutland and Lincolnshire (where it attains a thickness exceeding 200 feet) and into Yorkshire, which limestone has been distinguished by Mr. Judd as the "Lincolnshire limestone." The object of the author was to show that these two limestones were distinct, and that while the former was of the Great Oolite period, the latter as certainly belonged to the Inferior Oolite; and in citing evidence in proof of this position upon stratigraphical and palæontological grounds, he gave a general account of the geology of the northern division of Northamptonshire, illustrating his description by the exhibition of numerous fossils gathered from the various beds and localities referred to.

Between Northampton and Kettering, the Great Oolite limestone is the surface rock; and intersecting valleys upon that line, and the escarpment of the Ise valley, a mile east of Kettering, exhibit this sequence of beds:—

Great Oolite	{ Limestone.
	{ Upper Estuarine Clays.

Inferior Oolite { Lower Estuarine Beds } Northampton
 { Ferruginous Beds } Sand.
 Upper Lias Clay.

And this section, with the successive superaddition of Great Oolite Clay, Cornbrash, Kelloway Rock, and Oxford Clay, is continued due east across the country to the valley of the Nene, and on into Huntingdonshire.

Upon the same Ise escarpment, about a mile north-east of Kettering, the thin end of the wedge of the Lincolnshire limestone is seen to come in; and this sequence, for the first time, is presented:—

Great Oolite { Limestone.
 { Upper Estuarine Clays.
 Inferior Oolite { LINCOLNSHIRE LIMESTONE (very thin).
 { Lower Estuarine Beds } Northampton
 { Ferruginous Beds } Sand.
 Upper Lias Clay.

The same sequence, with the occasional superaddition of the Great Oolite Clay, was shown to be repeated upon the western escarpment of the Ise, at Glendon, Barford Bridge, near Rockingham, at Weekly, and at Geddington (the Lincolnshire limestone increasing in thickness at every advance), and to occur over and over again upon innumerable escarpments in the counties of Northampton, Rutland, Lincoln, and York, offering unmistakable and incontrovertible evidence of the true stratigraphical position of the Lincolnshire limestone.

The author described the section in the ancient quarries at Weldon, whence is obtained the highly reputed freestone of that name. He exhibited a diagram of the horizontal section from Rockingham, on the Welland valley (which marks to the north-west the line of division between Northamptonshire and Rutland), through Weldon, by Oundle, and across the Nene valley into Huntingdonshire. It was shown by this diagram that, although the Lincolnshire limestone has a thickness of some 30 feet at Weldon, it thins out very rapidly eastwards, being nowhere found within three miles of Weldon, and being altogether absent from the Nene escarpment at Oundle, which presents, with this exception, the whole series of beds from the Oxford Clay to the Upper Lias Clay inclusive.

The author successively described sections in the Lincolnshire limestone:—at Kirby, where the equivalent of the Colleyweston slate is quarried, this being the extreme western point at which it has been found; at Deene, Wakerley, Morecot (where the whole series from the Cornbrash to the Upper Lias inclusive occurs, the Lincolnshire limestone attaining to a thickness of 60 feet), Ketton (where the famous Ketton freestone is quarried), and Colleyweston. At the last-named place the well-known calcareo-arenaceous slate has been quarried for more than 350 years; and the numerous quarries occupy an area exceeding a mile in diameter.* Here everywhere may be noted the position of this slate-bed, passing the Lincolnshire limestone series, and immediately overlying the Lower Estuarine Sands.

Some three miles north-east of the Colleyweston slate quarries is the high ground of the Stamford Open Field, the summit of which is about 200 feet above the level of the river Welland. In this one hill-mass occur (and may be observed) the whole series of beds from the Cornbrash to the Upper Lias inclusive, the Lincolnshire limestone having reached a thickness of 75 feet, and being the most important bed of the section.

The limestone which occurs beneath the Great Oolite Clay and above the Upper Estuarine series high up upon the escarpment of Stamford Field, occurs also, at a level some 100 feet lower, at Bemisthorpe, and at Danes' Hill and Essendine, in cuttings of the Great Northern Railway, the only record of the sections of which is to be found in Professor Morris's paper in the Society's Journal for 1853. This limestone has not been considered to be Great Oolite at all; but the author showed, by a comparison of the whole group of fossils obtained from both beds, that this limestone was identical with the Great Oolite limestone of the Northampton district.

The author described the peculiar effect of a fault occurring south of the Welland at Stamford, by which the Upper Lias capped by the Northampton Sand has been thrown up to an elevation overtopping the town. Upon a severed and subsided mass the important suburb of St. Martin's has been built, and a peculiar repetition of beds has resulted—Upper Lias, Northampton Sand, Colleyweston Slate, and Lincolnshire Limestone being in a double sequence encountered upon an ascent of the escarpment. The fact was stated that the Colleyweston slate was found near the foot of the escarpment (whence the beautiful *Astropecten Cotteswoldiae*, var. *Stamfordensis*, Wright, was obtained by the author in 1853), and again upon the summit of the escarpment, at a distance of one third of a mile, at an increased elevation of 150 feet. Other anomalous results of the same fault, which extends some miles eastward, were described.

The area of the old "Barnack Rag" quarries was referred to. These were in work in the time of the Romans, but had been exhausted for 400 years. The stone (Lincolnshire limestone) was the building-stone over a large district in ancient times; and its excellence was proved by time.

The escarpment south of the Nene valley at Wansford presents the same sequence as that of the Stamford Field—Cornbrash to the Upper Lias inclusive. The railway-tunnel is excavated in the Lincolnshire limestone. East of this, at Castor and Water Newton, and west of the same point at Elton, the Lincolnshire limestone thins away, and the two estuarine series again come together in vertical contact. Along the Nene valley towards Northampton the southern escarpment presents the complete sequence of beds from the Oxford Clay to the Upper Lias inclusive (*minus the Lincolnshire limestone*), as far as Thrapstone. Here the Oxford Clay and Cornbrash part company and trend southward, the other members of the series (Great Oolite Clay and limestone, Upper Estuarine, Lower Estuarine, and ferruginous beds of Northampton sand, and Upper Lias) continuing on to the Northampton district.

The author contended that the stratigraphical and palæonto-

logical evidence combined to establish the position which he assumed in his introduction—that the Great Oolite limestone of Northampton was identical with what he considered was Great Oolite at Stamford, and that the “Lincolnshire limestone” was a distinct formation, and a member of the Inferior Oolite series.

He had confidence that he had shown that the series of beds in the north-eastern portion of the northern division of Northamptonshire comprised all the beds between the Oxford Clay and Upper Lias inclusive, *including the Lincolnshire limestone*—and that those of the south-western portion comprised the same sequence, *excluding the Lincolnshire limestone*.

He considered that the Great Oolite Clay represented the Forest Marble and the Bradford Clay of the West of England; that the Great Oolite limestone was nearly equivalent to the Great Oolite of Bath and the Cotteswolds, and to the upper beds of Minchinhampton; that the Upper Estuarine might be nearly identical with the Upper Plant Shale of Yorkshire, but more certainly with the Stonesfield slate of Oxfordshire; that the Lincolnshire limestone was nearly synchronous with the grey limestone of Yorkshire (Inferior Oolite), and probably with the lower portion of the *Am. Humphriesianus* zone of the west of England, but extending a little below this zone; that the Lower Estuarine answered to the Lower Plant Shale of Yorkshire, but had no representative in the west; that the upper portion of the ferruginous beds of the Northampton Sand was nearly upon the same horizon as the Glaizedale and Dogger beds of Yorkshire and the *Am. Murchisonæ* zone of the west; and that the lower portion of the Northampton sand was represented by the *Am. opalinus* zone and the Midford Sand.

XI. Intelligence and Miscellaneous Articles.

ON SOME PHENOMENA OF ILLUMINATION. BY A. LALLEMAND.

ALL the effects of illumination observed in diaphanous bodies traversed by the light of the sun, either natural or polarized, are readily explained if it be admitted that the vibratory motion of the æther in penetrating the transparent medium, encounters a resistance in virtue of which the vibrations are propagated laterally in such a manner that along any direction oblique to the incident ray the motion of the æthereal particle represents the projection of that of the æther on its passage from the luminous pencil, and if, on the other hand, it be also admitted that the molecules of the medium, absorbing a portion of the *vis viva* of the æther, vibrate in their turn and propagate in the æthereal fluid the complex vibrations which constitute natural light. The illumination therefore results from two effects superposed; and the light emanating from it is formed of two sorts of rays: the one sort, always of the same colour as the incident rays, are polarized either partially or completely, according as the incident pencil is neutral or polarized; the others, the refrangibility of which is often inferior to that of the exciting rays, have the properties of natural light, and determine a general property of bodies which has been called *fluorescence*. In

the case of opaque bodies this property corresponds to what is more usually designated by "the *proper colour* of the body."

I have just said that fluorescence is a general phenomenon of transparent bodies. In fact, by operating on the purest liquids (such, for example, as are obtained by condensing a gas like sulphurous acid, cyanogen, &c.), it is proved that the illumination is not extinguished when we look in a direction normal to the pencil and to the plane of polarization of the incident light; or if we view natural light through a double prism, one of the two images of the luminous ray never disappears completely. Nevertheless in studying under better conditions some crystallized substances, I have been able to recognize that quartz and rock-salt, when very pure, exhibit not the slightest trace of fluorescence and are illuminated very cleanly. To succeed in this experiment, it is necessary to polarize a large pencil of solar rays with a Foucault normal-face prism, and to concentrate it with a quartz lens cut parallel to the axis, of 35–40 centims. focus. The principal sections of the polarizer and the lens are made to coincide; and then their movements are rendered conjoint. If the pencil thus concentrated pass through the quartz either in the state of an ordinary or of an extraordinary ray, a very distinct white trail is observed in the plane of polarization, which a Nicol completely extinguishes. On looking in a direction normal to the plane of polarization, there is no illumination; there remains not the least trace of fluorescence. When the solar ray traverses the quartz along the optic axis, the dispersion of the plane of polarization has for its result the giving equal illumination around the ray, and polarization is complete only in a direction normal to the pencil. In this case chromatic illumination should be observed, similar to that of sirup of sugar and all solutions with rotatory power; but with quartz it is not manifest; indeed we know that the mixed tints, much weakened, all affect a uniform grey tint which the eye cannot distinguish. Very pure rock-salt is illuminated also like quartz, and is not fluorescent.

It is not the same with Iceland spar. All the specimens I have examined are illuminated with orange-red more or less brilliant; but this coloured illumination is the same in the plane of polarization and normal to that plane; it is not extinguished by a Nicol, when the emergent rays, which necessarily undergo double refraction, remain superposed. This red light is due solely to fluorescence; and the polarized illumination resembling that of quartz is not appreciable. A peculiarity which I must notice is, that when the incident solar thread is not polarized and traverses the spar rhombohedron so as to give two pencils well separated, the fluorescence due to the ordinary ray appears more vivid and of a deeper red than that of the extraordinary ray; at least this is what I have distinctly observed in two strongly fluorescent specimens. The fluorescence of spar has besides been studied by M. Becquerel with the phosphoroscope; and I only confirm, by another method, his observations. Colourless fluor-spar combines with more intensity the two distinct properties of quartz and of spar: it gives a white illumination, very bright in the plane of polarization, and a violet-

indigo fluorescence in the direction perpendicular to it. The three crystallized bodies quartz, spar, and fluorine, viewed with respect to the illumination, represent three types, to which all transparent bodies may be referred. To cite only one example, not yet remarked, pure naphthaline dissolved in alcohol or rectified essence of petroleum has a quinic fluorescence of a very bright indigo-blue. Spectral analysis of this light gives a very intense blue band extending from the line G to H and dominating the other colours of the spectrum, which it also includes.

I must also mention the curious effects obtained with prisms of chilled glass. The thread of polarized light which passes through them gives a luminous trace, white and partially polarized at certain points, while at others it is neutral and coloured yellowish green or bluish green, according to the fluorescence of the glass used. Without entering into further details, it is seen that these effects depend on the double refraction which the ray of light undergoes, and on the direction of the plane of polarization of the illuminating pencil.

To complete these observations I will add a few words on the photometric experiments by which I measured the proportion of polarized light contained in the rays emitted by a liquid illuminated by a pencil of natural light. If the liquid were not fluorescent, the polarization would be total when we look normally in any plane passing through the axis of the pencil, if it be admitted, as I have said above, that the trajectory of an æther particle on the visual line is not any thing else but the projection of the circle which is the envelope of all the ellipses with variable orientation which represent the motion of the æther in a ray of natural light.

The verification of this law would present no difficulty, if the inevitable fluorescence of the liquid did not add to the illumination a proportion of neutral light—constant, it is true, but of which it is necessary to take account.

I operated as follows, with a photometer the general arrangements of which reproduce those adopted by MM. Bernard and Edm. Becquerel. I view the illuminated pencil through a Nicol, of which the principal section is at first normal to it; and I make its light equal to that received from a lamp into a prism with total reflection after passing through two Nicols—the first movable, the second fixed, and their principal sections coinciding. This done, I extinguish the portion of polarized light emitted by the illuminated liquid by turning the first prism through 90° . In order to restore the equality of the lights, it is then only necessary to turn the movable Nicol through a certain angle, which serves to vary the intensity of the light for comparison.

Let α and α' be the angles of rotation which have restored equality of the images when the pencil was viewed, first normally, and then in a direction making an angle ω with the axis of the illuminating pencil. If f denote the proportion of fluorescent light, and m the light totally or partially polarized which comes from the lateral propagation of the luminiferous motion, we shall have the following equalities:—

$$\frac{f}{m+f} = \cos^2 \alpha, \quad \frac{m \cos^2 \omega + f}{m+f} = \cos^2 \alpha'.$$

Let it be remarked that we may suppose $m=1$, and that the two terms of the second ratio should be multiplied by the same factor variable with ω , since the light emitted varies with the depth of the luminous thread, and this changes with the inclination. Eliminating f in these two equalities, we get

$$\sin \alpha' = \sin \alpha \sin \omega.$$

I have verified this relation with a thin-walled spherical balloon filled successively with very pure alcohol and hydride of hexyl. Without entering into the detail of the experiments and the precautions taken to realize the equality of the tints of the two images (a condition without which the equality of the lights becomes illusory), I may say that the law was very well verified on causing ω to vary from zero to 65° ; the errors in the determinations of α never exceeded 1° , which exhibits a very sufficient approximation for photometric measures.—*Comptes Rendus de l'Académie des Sciences*, vol. lxxvii. pp. 1216–1219.

ON A PROCESS FOR VERIFYING THE NODES IN A SOUNDING PIPE.

BY M. BOURBOUZE.

The nodes of vibration in pipes are the places where the air is motionless, but where it undergoes alternate compressions and dilatations synchronous with the duration of the vibration. They are usually verified by showing that a membrane covered with sand, introduced into the tube, does not vibrate.

M. Kœnig has contrived placing in the side of the pipe a capsule closed interiorly by a flexible membrane, and through which circulates a current of illuminating gas, which is lighted. When the pipe carries the compressed membrane, it alternately dilates the current of carburetted hydrogen, and the flame undergoes oscillations which are ascertained by viewing them in a revolving mirror. This procedure is excellent, but does not lend itself to the projections which are necessary in lectures.

I replace these capsules by a simple membrane of flexible caoutchouc, upon which I fix a very light silvered mirror, so that it oscillates with the membrane. Consequently, if the rays from a luminous point be thrown upon this mirror, and the image be projected by a lens, the image will be seen to elongate, as in the experiments of M. Lissajous, and frequently to be transformed into an ellipse. It has its maximum of elongation when the mirror is at the node; it approaches immobility, and at length remains motionless, as the mirror is removed from the node to be placed on a loop.

This membrane can be placed at the extremity of a Helmholtz resonator, or at the end of a caoutchouc tube fixed to the extremity of that instrument; and we can assure ourselves that the mirror vibrates when we produce in the vicinity a mixed sound containing the note proper to the resonator.

The new process replaces with advantage, in lectures and in researches of investigation, those which have till now been made use of.—*Comptes Rendus de l'Académie des Sciences*, vol. lxxvii. p. 1099.

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XII. *On the Manufacture and Theory of Diffraction-gratings.*
By LORD RAYLEIGH, F.R.S.*

IN a "Preliminary Note on the Reproduction of Diffraction-gratings by means of Photography," published in the Proceedings of the Royal Society for June 20, 1872, and in the Philosophical Magazine for November of the same year, I gave a short account of experiments with which I had been for some time occupied. A few further details were communicated to the British Association at Brighton (Brit. Assoc. Report, p. 39). I now propose to give the results of more recent experience in the practical manufacture of gratings, as well as some theoretical conclusions which have been in manuscript since the subject first engaged my attention.

There are two distinct methods of copying practised by the photographer—(1) by means of the camera, (2) by contact-printing. The first, if it were practicable for our purpose, would have the advantage of leaving the scale arbitrary, so that copies of varying degrees of fineness might be taken from the same original. By this method I have obtained a photograph of a piece of striped stuff on such a scale that there was room for about 200 lines in front of the pupil of the eye, capable of showing lateral images of a candle; but I soon found that the inherent imperfections of our optical appliances, if not the laws of light themselves, interposed an almost insuperable obstacle to obtaining adequate results.

However perfect a lens may be, there is a limit to its powers of condensing light into a point. Even if the source from which

* Communicated by the Author.

the light proceeds be infinitely small, the image still consists of a spot of finite size surrounded by dark and bright rings. That this must be so may be shown by general considerations without any calculations. If a lens is absolutely free from aberration, the secondary waves issuing from the different parts of its hinder surface agree perfectly in phase at the focal point. Let us consider the illumination at a neighbouring point in the focal plane. If the distance between the two points is so small that the difference of the distances between the point under consideration and the nearest and furthest parts of the object-glass is but a small fraction of the wave-length (λ), the group of secondary waves are still sensibly in agreement, and therefore give a resultant illumination the same as before. At a certain distance from the focal point the secondary waves divide themselves into two mutually destructive groups, corresponding to the nearer and further parts of the object-glass. There is therefore here a dark ring. Further out there is again light, then another dark ring, and so on, the intensity of the bright rings, however, rapidly diminishing.

The radius r of the first dark ring subtends at the centre of the lens an angle θ given by

$$\sin \theta = \cdot 61 \frac{\lambda}{R}^*,$$

where R is the radius of the lens. If f be the focal length, we have

$$r = \cdot 61 \frac{f\lambda}{R}.$$

Let us now suppose that the problem is to cover a square inch with 3000 lines. On account of the curvature of the field it would be impossible to obtain extreme definition over the surface of a square inch with a less focal distance than (say) four inches.

If we take $f=4$ and $\lambda = \frac{1}{40,000}$, we find

$$R = \frac{\cdot 61}{10,000r},$$

which gives $R = \cdot 2$ for $r = \frac{1}{3000}$. That is to say, if the focal length were 4 inches and aperture $\cdot 4$ inch, the first dark ring corresponding to one of the lines would fall on the focal point of the neighbouring one—a state of things apparently inconsistent with good definition. It is true that the aperture might well be greater than half an inch, so that it may seem possible to satisfy the requirements of the case. But the result of the

* Verdet, *Leçons d'Optique Physique*, vol. i. p. 305.

above calculation, being founded on the supposition of entire freedom from aberration, both spherical and chromatic, is subject in practice to a large modification. In astronomical telescopes, where every thing is sacrificed to the requirement of extreme definition at the centre of the field, the theoretical limit is sometimes closely approached; but the case is very different with a photographic lens. In fact the very first thing it occurs to a photographer to do, when he wishes to improve the definition, is to contract the aperture of his lens by means of a stop—a course which would be attended with the opposite result in the case of a perfect object-glass, or even a good astronomical telescope. While, therefore, it might be too much to say that the reproduction of 3000 lines in an inch by lens and camera is impossible, the attempt to do so without very special appliances appears in a high degree unpromising. It would certainly require a lens more than usually free from spherical aberration, and unlike either a telescopic or a photographic object-glass*, achromatic (if the expression may be allowed) for the chemical rays, unless indeed the latter requirement could be evaded by using approximately homogeneous light. It must be understood that nothing is here said against the practicability of covering a small space with lines *at the rate of* 3000 to the inch, a feat probably well within the powers of a good microscopic object-glass.

The method of contact-printing, on the other hand, is free from optical difficulties. The photographic film prepared on a flat piece of glass (or other support) may be brought by moderate pressure in a printing-frame within a very short distance of the lines of the original grating; and if the source of light be moderately small and the rays fall perpendicularly, the copy rarely fails in definition, unless through some photographic defect. When direct processes not depending on development are employed, the unclouded light of the sun is necessary. To avoid too much diffused light, I usually place the printing-frame on the floor of a room into which the sun shines, and adjust its position until the light reflected from the plate-glass front is sent back approximately in the direction of the sun. Too much time should not be lost in this operation, which requires no particular precision. Usually I cut off part of the extraneous light by partially closing the shutters; but I cannot say whether this makes any difference in the result. Those who are accustomed to this kind of experimenting will know that it is often less trouble to take a precaution than to find out whether it is really

* Photographic lenses are corrected on the principle of making the "visual and chemical foci" coincident, which leads to a different construction from what would be adopted were the chemical rays alone attended to.

necessary. In an early stage of an investigation, when the causes of failure are numerous and unknown, it is best to exclude every thing that can possibly be supposed to be prejudicial. When the principal difficulties have been overcome, it will be time enough to determine what precautions are necessary, if the question has not been already settled by accidental experience.

In the case of developed plates there is more choice of lights in consequence of the higher sensitiveness. I have used successfully cloud or skylight reflected horizontally from the zenith by a mirror through a hole of two or three inches diameter in the shutter of a darkened room, the frame being set up in a vertical plane at a few feet distance. The principal objection to this plan is the difficulty of estimating the exposure with proper precision—a difficulty which is more felt than in ordinary photography, as it is convenient to develop a good many copies at once. On a really fine day the image of the sun formed by a condensing-lens of short focus placed in the shutter (as used in diffraction experiments) constitutes a very convenient source of light. As the exposure is only a few seconds, there is no difficulty in dodging isolated clouds, whose progress may be watched from within by examining their *image* with a coloured glass. When there is any haze, this method is not more satisfactory than the other.

With the more sensitive processes artificial light may be employed. I have done a good many copies by the aid of a moderator-lamp (without the globe) at two feet distance from the frame. An Argand gas-flame would probably be still better.

The printing-frame I employ has a thick plate-glass front, against which the original grating and the prepared plate are pressed by screws. These are more under control than the springs generally used in the common printing-frames. When every thing is ready, the original is placed on the glass front of the frame with the engraved face upwards, care being taken to exclude all grit by means of a camel's-hair brush. The prepared plate is then placed face downwards on the grating, then a pad to equalize the pressure (I have used one of india-rubber), and on the pad the rigid back of the frame, on which the screws are made to press with a moderate force. When the film is delicate, care should be taken to place it in the proper position at once without sliding.

The two surfaces of the plate-glass front of the printing-frame and the back of the original grating may be cleaned in the ordinary way with a soft cloth or wash-leather; but the engraved face of the grating requires more delicate treatment. If touched at all with a solid (wash-leather), the greatest care should be used. I prefer to wash it, when soiled, with a stream of water

from a tap, afterwards flooding it with pure alcohol and setting it up to drain and dry spontaneously. Sometimes I have found nitric acid useful; but I always try to avoid the rubbing contact of a solid. These precautions have been so successful that, after several hundred copies have been taken, the originals have scarcely, if at all, deteriorated.

For the support of the photographic film it is no doubt most satisfactory to use optically worked "parallel" glass. Ordinary glass would fail, for two reasons. In the first place it would generally be impossible to secure a sufficiently close contact in the printing. But even if this difficulty could be surmounted, the spectrum given by the copy would not bear the magnifying-power which it is generally desirable to apply. It is indeed evident that the glass support of the grating requires the same precision of workmanship as the object-glass of the telescope used in conjunction with it.

Although ordinary glass taken at random is inadmissible, I have done a great number of excellent gratings on selected pieces of patent plate. In order to choose the best, I lay the plates on a table in such a position that the bars of a window or skylight are seen reflected in them. Each bar appears in general double, one image corresponding to each surface. By sliding the plate about, while the head is kept still, irregularities are easily detected by the shifting or curvature of the images. From a package of two dozen 5×4 plates as issued by photographic dealers, three or four, often lying together, may usually be selected as flat enough for the purpose, or at any rate decidedly superior to the remainder. It is worth notice that the object aimed at is *flatness* of the two faces, exact *parallelism* being of much less consequence; for it is evident that the interposition of a truly worked prism of very acute angle would produce no evil result. A glass is therefore not to be rejected merely because the two images of the bar seen reflected in it are decidedly separated. The question is rather whether this separation remains constant as the plate is moved about without rotation. I have never seen a piece of patent plate that could not be at once distinguished from worked glass in the way described; so that the test is abundantly sufficient for the purpose. The more delicate methods by which worked glass is examined would be less practically useful.

Whatever kind of glass be used, if the photographic process be at all complicated, there is considerable economy of labour in preparing comparatively large pieces, to be afterwards cut with the diamond to the required size. A 5×4 , or even a $4\frac{1}{4} \times 3\frac{1}{4}$ plate will do very well for four gratings. In the case of worked glass economy is an object; but when patent plate is used I

should recommend 5×4 glasses, as a margin is convenient. Even when, as in the collodio-chloride process to be presently described, the plate for each grating is prepared separately, it is convenient to perform the preliminary operations of cleaning and albumenizing on larger pieces. The cutting of prepared plates requires a little care. I place them face downwards on a sheet of clean paper, make the diamond cuts on the back, and then, before breaking, remove as much as possible of the glass powder. As it is important to prevent any grit from getting between the film and the engraved face of the original, I usually brush the surface with a large camel's-hair brush kept scrupulously clean.

In the preparation of the plates I have used a considerable variety of methods. The process with gelatine and bichromate of potash described in my previous papers has decided advantages; but all my efforts to obtain a mastery of it have been unavailing. Plates prepared to all appearance in precisely the same manner, and even at the same time, turned out differently, while modifications purposely introduced seemed to be for the most part without effect. It required a strong scientific prejudice to hold the uniformity of nature in the face of so much adverse evidence. The uncertainty of this method is provoking, as some of the results are exceedingly good; but I gave up my attempts sooner than I might otherwise have done in consequence of the discovery of another method by which most of the advantages of the gelatine process, namely simplicity of manipulation and brilliancy of results, might be attained with much less risk of failure.

It is very possible that a photographer skilled in the employment of gelatine might succeed where I failed. In case any such should wish to make the attempt, I will mention a few points that seemed important. The solution of gelatine should be carefully filtered. For thick liquids containing gelatine, albumen, &c., the best filtering material that I know of is tow. The tow should be cleaned from grease by boiling with soda and subsequent washing, and a small plugget of it pushed with moderate force into the neck of the funnel. Some arrangement must be adopted for keeping the gelatine hot, or the operation will hardly succeed. It is important that the coat of gelatine should be even, for which object the glass must be free from grease, and the plate on which the prepared glasses are put away to set perfectly level. Even then a good deal depends on the manipulation; but this is soon learned. The uniformity of the coat may be tested by the colour when the plate is placed on a sheet of white paper and examined in a weak white light. By candle-light the colour of weak bichromate of potash is scarcely visible.

The exposure may be from two to six minutes to the direct rays of the sun. I have not been able to detect any deterioration when the plates were kept a few days in the dark before being used.

A photographer accustomed to either the plain albumen or the Taupenot process will find it very suitable for gratings. The hardness of the surface, which allows varnish to be dispensed with, is a great advantage. In my experiments with plain albumen, the principal difficulty was the purely photographic one of avoiding stains. It must be observed, however, that in actual use the gratings are not seen in focus, and that excellent spectra may be obtained from copies which a photographer would be inclined to throw away at once as hopelessly faint and dirty. The objection to the Taupenot process is the trouble of preparing the plates; but this is much mitigated when the plan is adopted of preparing large pieces to be afterwards cut up.

Among those requiring development, the tannin process is the one with which I have been most successful. In order to counteract the well-known tendency of the film to loosen, a preliminary coating of dilute albumen or gelatine is generally necessary. In the production of gratings the photographer must not be satisfied with merely keeping the film on the glass; the slightest tendency to looseness must be considered highly objectionable. The plates are coated with Mawson's collodion, excited in an ordinary silver-bath, washed first in distilled water and afterwards under the tap, and then immersed for a minute in a well-filtered 15-grain solution of tannin. On removal from the tannin, they are set up cornerwise on blotting-paper to drain and dry.

For the development of these plates I prefer a solution of gallic acid employed in a dish, though I have obtained very good results by the ordinary method with pyrogallic acid. Prepare the two following:—

- | | | |
|-----|-------------------------|-------------|
| (1) | Gallic acid | 100 grains. |
| | Alcohol | 2½ oz. |
| | Distilled water . . . | 2½ oz. |
| (2) | Nitrate of silver . . | 100 grains. |
| | Glacial acetic acid . . | 2 oz. |
| | Distilled water . . . | 16 oz. |

The dish used for developing should be of glass, and is best cleaned with a little strong nitric acid, which may be used over and over again. The developing solution is prepared by mixing (1) and (2) in equal parts and diluting with water to half the strength. The alcohol helps to keep the film tight; and the development is well under control. In warm weather the opera-

tion may take an hour; but much depends upon the exposure, and still more upon the temperature. The proper point to which to carry the development can only be learned by experience; but the beginner is most likely to err on the side of excess, particularly if he uses pyrogallic acid. If, as is desirable, the film be creamy and thick, the spectra of a candle do not appear to advantage at this stage, in consequence of the unaltered iodide of silver. For fixing, "hypo" is the safest, though cyanide may be used if the film will bear it.

Tannin plates when finished are hardly secure without varnish; but there is considerable risk of spoiling gratings in the operation if an ordinary negative varnish be used. The crystal (benzole) varnish, which is applied cold, is much easier to use and gives adequate protection.

But the process which I am now most inclined to recommend is that introduced by Mr. Wharton Simpson, and known as the collodio-chloride process. The collodion, which may be procured ready for use from Messrs. Mawson and Swan, of Newcastle, consists of an emulsion of finely divided chloride of silver held in suspension by the dissolved gun-cotton, together with a carefully adjusted excess of free nitrate. After a time the chloride of silver is precipitated and the preparation becomes useless; but if properly mixed in the first instance, it will remain fit for use for weeks or even months. In the production of gratings the consumption is very small; so that, if required for this purpose alone, it is well to order it in small quantities.

In order to secure a proper adhesion, I have found a preliminary coating of albumen absolutely necessary. The white of an egg beaten up with a pint of distilled water gives a solution of sufficient strength. The plates, previously cleaned, are coated in any way that may be found convenient, and then set up on blotting-paper to drain and dry. The principal precautions necessary are to filter the albumen very carefully and to work in a room free from dust. It will generally be convenient to prepare a considerable number of plates at a time. Though of almost infinitesimal thickness, the film of albumen produces a very marked effect. Without the albumen the skin of collodion will usually come right away from the glass when washed under the tap; with it the adhesion is remarkably good, and the film so tough as even to bear rubbing with the finger while wet.

The plate is coated with collodion in the ordinary way, and, after resting a few seconds, is dried by heat over a spirit-lamp or otherwise. After the plate has been made quite warm, it is put aside in the dark to cool and to absorb a certain amount of moisture from the atmosphere. This may take five or ten minutes. If the plate is used too soon the result is unsatisfactory;

but, on the other hand, it will not do to leave it long enough to become sensibly moist. Something will probably depend on the particular sample of collodio-chloride.

The exposure required is about five or seven minutes to the autumn sun. On a hazy day something more may be required; but if there are many clouds about, the experimenter, unless naturally of a good temper, will do well to postpone his operations.

On removal from the frame, the plates may be placed in a dish of water until it is convenient to finish them. They are fixed, without any toning, in a dilute solution of hyposulphite of soda, such as is used for paper prints, and then carefully washed. The most effective washing is a combination of rinsing and soaking. My practice is to rinse the plates under the tap for half a minute in order to remove the greater part of the hyposulphite of soda, and then to allow them to soak for an hour or two in water changed two or three times. After a final rinsing the plates may be set up to dry.

Gratings finished in this way give excellent definition, but the spectra are rather deficient in brilliancy. This defect is of less importance than might be supposed; for in order to see the finer fixed lines, sunlight is in any case indispensable, and with sunlight there is usually illumination to spare. Nevertheless, as gratings are likely to be largely used for the purpose of popular illustration under circumstances where artificial light must be employed, I am glad to be in a position to recommend a simple mode of treatment by means of which the brilliancy of the spectra may be materially enhanced. For this purpose it is only necessary to treat the fixed and washed impression with a solution of corrosive sublimate. When the whitening effect is complete, the plate must be again washed and then set up to dry. Considered as a photographic transparency, the grating is reduced rather than heightened in intensity by this process. The cause of the improvement of the spectra will be touched upon presently. These mercury-treated gratings cannot be varnished without sacrificing most of the advantage of the method. I have occasionally applied the same treatment successfully to tannin plates.

When not in use, the finished gratings should be kept in a dry place and protected from dust and other atmospheric deposit. For this purpose they may be put away wrapped in paper. For a short time there is no objection to leaving them standing, face inwards, against a wall; but a better plan is to place them, face downwards, on a flat and thoroughly clean piece of plate glass.

The originals from which I have hitherto taken copies are three in number. Two are by Nobert, and contain respectively 3000 and 6000 lines, in each case covering a square inch (Paris).

On a casual inspection the second, apart from the greater number of its lines, would be preferred as presenting a more even appearance. The 3000-line grating is divided into three parts, giving spectra of differing degrees of brightness, corresponding no doubt to a variation in the cut of the diamond or other stone employed, a peculiarity which is faithfully preserved in the copies. But on actual trial it is found that the spectra of the 3000-line grating are much the best in respect of definition; and the same difference is observed in the copies. The superior brilliancy of the closer-ruled grating is thus of little or no advantage for the investigation of the solar spectrum. In order to make good use of it, a higher degree of magnifying-power would be necessary than the definition of the spectra will bear.

The other original grating was engraved by Mr. Rutherford, of New York, and was kindly lent me by Mr. Browning; it contains 6000 lines to the inch. Owing to a change of residence, I have not hitherto had an opportunity of testing either the original or the copies on the solar spectrum; but I may observe that in respect of brightness they fall far short of Nobert's. This, as I have already remarked, is not always an objection; and the accuracy of division, on which definition depends, is said to be very superior*.

In testing gratings I prefer to work in a dark room. The slit is fastened in the window-shutter, outside which is placed the heliostat or *porte-lumière*. As slits are frequently required in optical experiments, and as usually made are rather expensive, I may be allowed to mention a very simple method by which serviceable slits may often be obtained. A piece of glass is covered with tinfoil, which must be made to adhere well; I have found a weak shellac-varnish a suitable cement. The alcohol is allowed to evaporate, and the thin layer of shellac softened by heat. In order to make a slit, it is only necessary to lay a straight edge on the tinfoil and draw a line with a sharp knife, afterwards wiping the line of the cut with a rag moistened with alcohol. The width and regularity of the slit may be judged of by holding it close to the eye, and observing the appearance presented by a distant candle. The narrower the aperture the more dilated (in the direction of the width of the slit) the image will appear. Broader slits may be made by removing the foil between two parallel cuts.

At a distance of 12 feet or more from the shutter are placed the grating and the object-glass of the observing-telescope. In making the preliminary adjustments, it is convenient to use a slit so wide that the spectra and the light reflected from the

* Draper, "On Diffraction-Spectrum Photography," *Phil. Mag.* Dec. 1873, p. 419.

grating can be seen on a screen. By the second the aspect of the plane of the grating can be judged of; and when the line of spectra is horizontal, it will be known that the lines of the grating are vertical and parallel to the slit. As object-glass, I am in the habit of using a single lens of about 24 inches solar focus. The eyepiece is a high-power achromatic, supplied by Mr. Browning, and forms, with the object-glass, a telescope of much higher magnifying-power than is ordinarily used in spectroscopes. Without a high power it is impossible to bring out the full value of the grating. In order to obtain the best definition, it is necessary to adjust carefully the aspect of the object-glass; and I find that the best aspect is not always the same. It is possible that the performance of other optical instruments might occasionally be improved if means were provided for a slight alteration in the direction of the optic axis of one of the lenses employed. The grating itself I usually place approximately in the position of minimum deviation.

The copies on worked glass by the ordinary photographic processes and by the modification of the collodio-chloride last described rarely fail in definition. With the original (3000) grating, or with the copies, I can make out nearly, but not quite, all that is shown in Ångström's map. With this grating the third spectrum is generally the most serviceable. When the picked patent plate is employed, there will generally be a proportion whose performance is less satisfactory, though few which would not give very fair results when tested by a low power only. Some cannot be considered inferior to the worked glass, at least when the object-glass is specially adjusted for them. In many cases the definition may be considerably improved by the use of a diaphragm in the form of a *horizontal* slit, so placed that only the central parts of the lines of the grating are operative. In respect of brilliancy, gratings may be more quickly judged of; it is sufficient merely to examine the spectra of a candle placed in a dark room.

The lines themselves are of course too close to be seen without a microscope; but their presence may be detected, and even the interval between them measured, without optical aid, by a method not depending on the production of spectra or requiring a knowledge of the wave-length of light. If two photographic copies containing the same number of lines to the inch be placed in contact, film to film, in such a manner that the lines are nearly parallel in the two gratings, a system of parallel bars develops itself, whose direction bisects the external angle between the directions of the original lines, and whose distance increases as the angle of inclination diminishes. The cause of the phenomenon will be readily understood by drawing on paper two

sets of equally distant and not too thin bars inclined at a small angle. Where the opaque and transparent parts severally overlap, the obstruction of light is, on the average, less than the double of that due to each set separately*, and consequently these places appear by comparison bright. The interval between the bars is evidently half the long diagonal of the rhombus formed by two pairs of consecutive lines, and is expressed by $a \cos \frac{1}{2} \theta \div \sin \theta$, or approximately $a \div \theta$, where a is the interval between the primary lines, and θ the mutual inclination of the two sets.

When parallelism is very closely approached, the bars become irregular, in consequence of the imperfection of the ruling. This phenomenon might perhaps be made useful as a test.

If the planes of the films be not quite parallel, bars parallel to the original lines may appear when the line of intersection of the planes is in the same direction. This arises from a foreshortening of one of the sets, making it equivalent to a grating of a somewhat higher degree of fineness.

When examined under the microscope, the opaque bar on the copy, which corresponds to the shadow of the groove of the original, is seen to be composite, being not unfrequently traversed along its length by several fine lines of transparency. In one case, where the copy was on common glass, this effect went so far that at certain parts of the grating the periodicity was altered by each line splitting into two, the first spectrum altogether disappearing. In order to make this observation, the eye should be placed at the point where the pure spectra are formed and be focused on the grating. The places in question will then appear as irregular dark bands.

The disappearance of the first spectrum is very unusual; but it is common for bands to appear when the eye is placed in the place of the fourth and higher spectra. When the order is high, the bands will not be black, but coloured with light belonging to one of the other spectra. There is no difficulty in understanding how this occurs. In the process of copying, the groove of the original is widened into a bar, whose width depends on the closeness of contact, an element which necessarily varies at different parts of the plate. The dark bands are the locus of points at which the relation of the alternate parts is such as to destroy the spectrum in question.

I have not had an opportunity of trying the method of copying on lines closer than 6000 to the inch; but I have no doubt that the limit of fineness was not attained. I should expect to find no difficulty with lines 10,000 or 12,000 to the inch; but beyond that point it is possible that the method would fail,

* The mathematical reader will easily prove this from the law of absorption.

or require special precautions, such as the use of extra-flat glass and greater pressure to ensure close contact in the printing. For preliminary experiments I should be inclined to try mica as a support, whose flexibility would facilitate a close contact. I may mention that I have done copies of the 3000-line grating on sheets of mica, which may be obtained very thin and smooth from the photographic dealers. For more convenient manipulation in the preliminary stages of preparation, the mica should be mounted on a sheet of glass of the same size as itself. A small drop of water interposed will ensure a sufficiently close adhesion.

I have tried to take copies of copies, but with indifferent success, even when the performance of the first was not perceptibly inferior to that of the original.

Gratings may be copied without the aid of photography by simply taking a *cast*. Following Brewster, I have obtained a fair result by allowing filtered gelatine to dry after being poured on the 3000 Nobert. This method, however, is attended with much more risk to the original, and is besides open to other objections, sufficient, I think, to prevent its competing with photography.

[To be continued.]

XIII. On Wheatstone's Bridge.

By OLIVER HEAVISIDE, Esq.

To the Editors of the Philosophical Magazine and Journal.

GENTLEMEN,

Newcastle-on-Tyne,
January 11, 1874.

MR. BROUGH, in the last Number of this Journal, has called attention to the fact that, in a paper "On the best Arrangement of Wheatstone's Bridge," in the *Philosophical Magazine* for February 1873, I did not start from the general equation for the strength of the current through the galvanometer irrespective of a balance, but took that form which the expression assumes when at a balance. In fact I assumed ($ad-bc$) to be indefinitely small. My reason for doing so was, that the question was to find the best arrangement *at* a balance, the great object of resistance-measuring being to *get* a balance; besides which, the course I adopted led to much greater simplicity and no less accuracy.

To take the special case upon which Mr. Brough dwells, viz. to find the best resistance for the galvanometer. This amounts to finding the resistance external to the galvanometer; and when a balance is obtained, as the galvanometer and battery are conju-

gate resistances, the resistance of the latter does not appear in the expression for the resistance external to the galvanometer, which is not in any way affected by the battery resistance. Similarly for the resistance external to the battery, which, at a balance, is independent of the galvanometer resistance. I cannot agree with Mr. Brough that to find the resistance in either case at a balance is a mere mathematical problem destitute of physical meaning; for it is only when at a balance that the problem has any practical importance.

As Mr. Brough most truly observes, most Wheatstone's bridges are wrongly arranged. An excellent example of this once came under my notice. A gentleman informed me he was about to make a Wheatstone's bridge, a great improvement, and very economical. Instead of using three separate sets of resistance-coils (a , b , c) he would use only one (c); for he would make a and b equal to 0. Mr. Harris's arrangement appeared to succeed admirably. There was no difficulty whatever in getting a zero; in fact there was always a balance, whether the line under examination was long or short. There was only one drawback; and that was, the improvement afforded no information whatever as to the resistance of the line.

I am &c.,

OLIVER HEAVISIDE.

P.S.—The condition that the galvanometer should connect the junction of the two greatest with the junction of the two least of the resistances, is necessarily complied with by the equations I have given for the best arrangement with a given galvanometer and battery; else it would not be the best arrangement.

XIV. *On Ocean-currents*.—Part III. *On the Physical Cause of Ocean-currents*. By JAMES CROLL, of the Geological Survey of Scotland.

[Continued from vol. xlii. p. 280.]

Further Examination of the Gravitation Theory of Oceanic Circulation.

Introduction.

FEW subjects have excited more interest and attention than the cause of ocean circulation; and yet few are in a more imperfect and unsatisfactory condition, nor is there any question regarding which a greater diversity of opinion has prevailed. Our incomplete acquaintance with the facts relating to the currents of the ocean and the modes of circulation actually in operation, is no doubt one reason for this state of things. But doubtless the principal cause of such diversity of opinion lies in

the fact that the question is one which properly belongs to the domain of physics and mechanics, while as yet no physicist of note (if we except Dr. Colding, of Copenhagen) has given, as far as I know, any special attention to the subject. It is true that in works of meteorology and physical geography reference is continually made to such eminent physicists as Herschel, Pouillet, Buff, and others; but when we turn to the writings of these authors we find merely a few remarks expressive of their opinions on the subject, and no special discussion or investigation of the matter, nor any thing which could warrant us in concluding that such investigations have ever been made. At present the question cannot be decided by a reference to authorities.

The various theories on the subject may be classed under two divisions: the first of these attributes the motion of the water to the *impulse of the wind*, and the second to the *force of gravity* resulting from difference of density. The latter may be subdivided into two classes. The first of these (of which Maury may be regarded as the representative) attributes the Gulf-stream and other sensible currents of the ocean to difference of specific gravity. The other class (at present the more popular of the two, and of which Dr. Carpenter may be considered the representative) denies altogether that such currents can be produced by difference of specific gravity*, and affirms that there is a general movement of the upper portion of the ocean from the equator to the poles, and a counter movement of the under portion from the poles to the equator. This movement is attributed to difference of specific gravity between equatorial and polar water, resulting from difference of temperature.

The former theory I examined at some length in a paper in the *Philosophical Magazine* for October 1870, and the latter theory in a paper in the same journal for October 1871. Since then Dr. Carpenter has done me the honour, in a paper read before the Royal Society†, to discuss at considerable length the various objections advanced by me to his theory. He has also in this memoir stated and explained his views on several points more fully than on former occasions. He further restates at some length the various facts for which his theory is designed to account, facts which he considers I have never attempted to explain. This to a certain extent is true; for as yet I have not reached that part of my paper "On Ocean-currents" in which these points fall to be discussed. One of the objects of the present paper is to endeavour to show that all the facts to which Dr. Carpenter refers can be perfectly well explained without having recourse to any such general movement of the ocean as he

* Proceedings of the Royal Society, No. 138, p. 596, foot-note.

† See Proc. Roy. Soc. No. 138.

assumes to exist. I have also considered more in detail what seem to me to be the radical defects of his theory, and have again reviewed some matters regarding which he appears to have slightly misapprehended the drift of my argument. It was shown on a former occasion that, if the heat received by the ocean in intertropical regions were distributed over the globe, not by currents produced by the wind, but by means of a circulation due to difference of temperature between equatorial and polar waters, then there could be no secular changes of climate resulting from variations in the eccentricity of the earth's orbit—because such a mode of circulation would, as I have shown, tend to neutralize the effects which would otherwise result from an increase of eccentricity. For this reason I have been the more anxious to prove that intertropical heat is conveyed to temperate and polar regions by ocean-currents, and not by means of any general movement of the ocean resulting from difference of gravity. I have therefore on this account entered more fully into that part of the subject than I otherwise would have done. Irrespective of all this, however, the important nature of the whole question, and the very general interest it excites, may be regarded as sufficient excuse for the length of the present communication. Circumstances over which I had no control have delayed its publication for nearly a year.

The Facts and their Explanation.

“I have thought it desirable,” says Dr. Carpenter, “to develop somewhat at length what I regard as the bearings of the results obtained by these inquiries upon the doctrine of a general oceanic circulation sustained by difference of temperature. . . . As no similarly comprehensive examination has been made, so far as I am aware, by any other scientific inquirer, and as the doctrine put forth on the subject by Mr. Croll is likely, if not thus scrutinized, to command the unquestioning assent of those who regard him as a high authority on the subject of oceanic currents and their bearings on geological questions, I venture to hope that the conclusion of its results as an appendix to this Report will not be deemed inappropriate” (p. 538).

The Facts to be explained.—He then commences by giving a restatement of the facts for the explanation of which his theory of a general oceanic circulation has been advanced. It is well known that, wherever temperature-observations have been made in the Atlantic, the bottom of that ocean has been found to be occupied by water of an ice-cold temperature. And this holds true not merely of the Atlantic, but also of the ocean in intertropical regions—a fact which has been proved by repeated observations, and more particularly of late by those of Commander Chimmo in the China

Sea and Indian Ocean, where a temperature as low as 32° Fahr. was found at a depth of 2656 fathoms. In short the North Atlantic, and probably the intertropical seas also, may be regarded, Dr. Carpenter considers, as divided horizontally into two great layers or strata—an upper warm, and a lower cold stratum. All these facts I, of course, freely admit; nor am I aware that their truth has been called in question by any one, no matter what his views may have been as to the mode in which they are to be explained.

The Explanation of the Facts.—We have next the explanation of the facts, which is simply this:—The cold water occupying the bottom of the Atlantic and of intertropical seas is to be accounted for by the supposition that *it came from the polar regions*. This is obvious, because the cold possessed by the water could not have been derived from the crust of the earth beneath: neither could it have come from the surface; for the temperature of the bottom water is far below the normal temperature of the latitude in which it is found. Consequently “the inference seems irresistible that this depression must be produced and maintained by the convection of cold from the polar towards the equatorial area.” Of course, if we suppose a flow of water from the poles towards the equator, we must necessarily infer a counter flow from the equator towards the poles; and while the water flowing from equatorial to polar regions will be *warm*, that flowing from polar to equatorial regions will be *cold*. The doctrine of a mutual interchange of equatorial and polar water is therefore a *necessary consequence* from the admission of the foregoing facts. With this *explanation of the facts* I need hardly say that I fully agree; nor am I aware that its correctness has ever been disputed. Dr. Carpenter surely cannot charge me with overlooking the fact of a mutual interchange of equatorial and polar water, seeing that my estimate of the thermal power of the Gulf-stream, from which it is proved that the amount of heat conveyed from equatorial to temperate and polar regions is enormously greater than had ever been anticipated, was made a considerable time before he began to write on the subject of oceanic circulation*. And in my paper “On Ocean-currents in relation to the Distribution of Heat over the Globe”†, I have endeavoured to show that, were it not for the raising of the temperature of polar and high temperate regions and the lowering of the temperature of intertropical regions by means of this interchange of water, these portions of the globe would not be habitable by the present existing orders of beings.

* Trans. of Glasgow Geol. Soc. for April 1867. Phil. Mag. for Feb. 1867 and June 1867 (Supplement).

† Phil. Mag. for February 1870.

The explanation goes further :—"It is along the surface and upper portion of the ocean that the equatorial waters flow towards the poles, and it is along the bottom and under portion of the ocean that polar waters flow towards the equator; or, in other words, the warm water keeps the *upper* portion of the ocean and the cold water the *under* portion." With this explanation I to a great extent agree. It is evident that, in reference to the northern hemisphere at least, the most of the water which flows from intertropical to polar regions (as, for example, the Gulf-stream) keeps to the surface and upper portion of the ocean; but, for reasons which I have stated in my last paper*, a very large proportion of this water must return in the form of *under* currents; or, which is the same thing, the return compensating current, whether it consist of the actual water which originally came from the equator or not, must flow towards the equator as an under current. That the cold water which is found at the bottom of the Atlantic and of intertropical seas must have come as under currents is perfectly obvious, because water which should come along the surface of the ocean from the polar regions would not be cold when it reached intertropical regions.

The explanation hypothetical.—Here the general agreement between us in a great measure terminates; for Dr. Carpenter is not satisfied with the explanation generally adopted by the advocates of the *wind theory*, viz. that the cold water found in temperate and intertropical areas comes from polar regions as compensating under currents, but advances a *hypothetical* form of circulation to account for the phenomenon. He assumes that there is a *general set* or flow of the surface and upper portion of the ocean from the equator to polar regions, and a *general set* or flow of the bottom and under portion of the ocean from polar regions to the equator. Mr. Ferrel ('Nature,' June 13, 1872) speaks of that "interchanging motion of the water between the equator and the pole *discovered* by Dr. Carpenter." In this, however, Mr. Ferrel is mistaken; for Dr. Carpenter not only makes no claim to any discovery of the kind, but distinctly admits that none such has yet been made. Although in some of his papers he speaks of a "*set* of warm surface-water in the southern oceans toward the Antarctic pole" as being well known to navigators, yet he nowhere affirms, as far as I know, that the existence of such a general oceanic circulation as he advocates has ever been directly determined from observations. This mode of circulation is *simply inferred* or *assumed* in order to account for the facts referred to above. "At present," Dr. Carpenter says, "I claim for it no higher character than that

* Phil. Mag. for October 1871, p. 267.

of a good working *hypothesis* to be used as a guide in further inquiry" (§ 16); and lest there should be any misapprehension on this point, he closes his memoir thus:—"At present, as I have already said, I claim for the doctrine of a general oceanic circulation no higher a character than that of a good working *hypothesis* consistent with our present knowledge of facts, and therefore entitled to be *provisionally* adopted for the purpose of stimulating and directing further inquiry."

I am unable to agree with Dr. Carpenter on this latter point. It seems to me that there is no necessity for adopting any hypothetical mode of circulation to account for the facts, as they can be quite well accounted for by means of that mode of circulation which does *actually exist*. It has been determined from direct observation that surface-currents flow from equatorial to polar regions; and their paths have been actually mapped out. But if it is established that currents flow from equatorial to polar regions, it is equally established that return currents flow from polar to equatorial regions; for if the one *actually* exists, the other of necessity *must* exist. We know also on physical grounds, to which I have already referred, and which fall to be considered more fully in a subsequent part of this paper, that a very large portion of the water flowing from polar to equatorial regions must be in the form of under currents. If there are cold under currents, therefore, flowing from polar to temperate and equatorial regions, this is all that we really require to account for the cold water which is found to occupy the bed of the ocean in those regions. It does not necessarily follow, because cold water may be found at the bottom of the ocean all along the equator, that there must be a direct flow from the polar regions to every point of the equator. Water brought constantly from the polar regions to various points along the equator by means of under currents will necessarily accumulate, and in course of time spread over the bottom of the intertropical seas. It must either do this, or the currents on reaching the equator must bend upwards and flow to the surface in an unbroken mass. Considerable portions of some of those currents may no doubt do so and join surface-currents; but probably the greater portion of the water coming from polar regions extends itself over the floor of the equatorial seas. In a letter in 'Nature,' Jan. 11, 1872, I endeavoured to show that the surface-currents of the ocean are not separate and independent of one another, but form one grand system of circulation, and that the impelling cause keeping up this system of circulation is not the *trade-winds* alone, as is generally supposed, but the *prevailing winds of the entire globe considered also as one grand system*. The evidence for this opinion, however, will be considered more fully in the next part of this paper.

Although the under currents are parts of one general system of oceanic circulation produced by the impulse of the system of prevailing winds, yet their direction and position are nevertheless to a large extent determined by different laws. The water at the surface, being moved by the force of the wind, will follow the path of *greatest pressure and traction*,—the effects resulting from the general contour of the land, which to a great extent are common to both sets of currents, not being taken into account; while, on the other hand, the under currents from polar regions (which to a great extent are simply “indraughts” compensating for the water drained from equatorial regions by the Gulf-stream and other surface-currents) will follow, as a general rule, the path of *least resistance*.

The Cause assigned for the hypothetical mode of circulation.—Dr. Carpenter assigns a cause for his mode of circulation; and that cause he finds in the difference of specific gravity between equatorial and polar waters, resulting from the difference of temperature between these two regions. “Two separate questions,” he says, “have to be considered, which have not, perhaps, been kept sufficiently distinct either by Mr. Croll or by myself:—*first*, whether there is adequate evidence of the existence of a general vertical oceanic circulation; and *second*, whether, supposing its existence to be provisionally admitted, a *vera causa* can be found for it in the difference of temperature between the oceanic waters of the polar and equatorial areas” (§ 17). It seems to me that the facts adduced by Dr. Carpenter do not necessarily require the assumption of any such mode of circulation as that advanced by him. The phenomena can be satisfactorily accounted for otherwise; and therefore there does not appear to be any necessity for considering whether his hypothesis be sufficient to produce the required effect or not.

An important consideration overlooked.—But there is one important consideration which Dr. Carpenter seems to have overlooked—namely, the fact that the sea is saltier in intertropical than in polar regions, and that this circumstance, so far as it goes, must tend to neutralize the effect of difference of temperature. It is probable indeed that the effect produced by difference of temperature is thus entirely neutralized, and that no difference of density whatever exists between the sea in intertropical and polar regions, and consequently that there is no difference of level nor any thing to produce such a general motion as he supposes. This I am glad to find is the opinion of Professor Wyville Thomson.

“I am greatly mistaken,” says that author, “if the low specific gravity of the polar sea, the result of the condensation and precipitation of vapour evaporated from the intertropical area,

do not fully counterbalance the contraction of the superficial film by arctic cold. . . . Speaking in the total absence of all reliable data, it is my general impression that if we were to set aside all other agencies, and to trust for an oceanic circulation to these conditions only which are relied upon by Dr. Carpenter, if there were any general circulation at all, which seems very problematical, the odds are rather in favour of a warm under-current travelling northwards by virtue of its excess of salt, balanced by a surface return-current of fresher though colder arctic water." ('The Depths of the Sea,' pp. 376 & 377.)

This is what actually takes place on the west and north-west of Spitzbergen. There the warm water of the Gulf-stream flows underneath the cold polar current. And it is the opinion of Dr. Scoresby, Clements Markham, and Lieut. Maury that this warm water, in virtue of its greater saltness, is denser than the polar water. Mr. Leigh Smith found on the north-west of Spitzbergen the temperature at 500 fathoms to be 52° , and once even 64° , while the water on the surface was only a degree or two above freezing*. Mr. Aitken, of Darroch, in a paper lately read before the Royal Scottish Society of Arts, showed experimentally that the polar water in regions where the ice is melting is actually less dense than the warm and more salt tropical waters. Nor will it help the matter in the least to maintain that difference of specific gravity is not the reason why the warm water of the Gulf-stream passes under the polar stream—because if differences of specific gravity be not the cause of the warm water underlying the cold water in polar regions, then difference of specific gravity may likewise not be the cause of the cold water underlying the warm at the equator; and if so, then there is no necessity for the gravitation hypothesis of oceanic circulation.

There is little doubt that the superheated stratum at the surface of the intertropical seas, which stratum, according to Dr. Carpenter, is of no great thickness, is less dense than the polar water; but if we take a column extending from the surface down to the bottom of the ocean, this column at the equator will be found to be as heavy as one of equal length in the polar area. And if this be the case, then there can be no difference of level between the equator and the poles, and no disturbance of static equilibrium nor any thing else to produce circulation.

Under currents account for all the Facts better than Dr. Carpenter's Hypothesis.—Assuming, for the present, the system of prevailing winds to be the true cause of oceanic currents, it necessarily follows (as will be shown hereafter) that a large quantity of Atlantic water must be propelled into the Arctic Ocean; and such, as we know, is actually the case. But the Arctic

* The Threshold of the Unknown Region, p. 95.

Ocean being, as Professor Wyville Thomson remarks, a well-nigh closed basin, not permitting of a free outflow into the Pacific Ocean of the water impelled into it, and the general tendency of the winds being to prevent or retard the return of the water into the Atlantic, the path of least resistance for the return currents must lie at a considerable distance below the surface. A large portion of the water from the polar regions no doubt leaves those regions as surface-currents; but a surface-current of this kind, on meeting with some resistance to its onward progress along the surface, will dip down and continue its course as an under current. We have an example of this in the case of the polar current, which upon meeting the Gulf-stream on the banks of Newfoundland divides—a portion of it dipping down and pursuing its course underneath that stream into the Gulf of Mexico and the Caribbean Sea. And that this under current is a real and tangible current, in the proper sense of the term, and not an imperceptible movement of the water, is proved by the fact that large icebergs deeply immersed in it are often carried southward with considerable velocity against both the force of the wind and the Gulf-stream.

Dr. Carpenter refers at considerable length (§ 134) to Mr. Mitchell's opinion as to the origin of the polar current, which is the same as that advanced by Maury, viz. that the impelling cause is difference of specific gravity. But although Dr. Carpenter quotes Mr. Mitchell's opinion, he nevertheless does not appear to adopt it; for in §§ 90–93 and various other places he distinctly states that he does not agree with Captain Maury's view that the Gulf-stream and polar current are caused by difference of density. In fact Dr. Carpenter seems particularly anxious that it should be clearly understood that he dissents from the theory maintained by Maury. But he does not merely deny that the Gulf-stream and polar current can be caused by difference of density; he even goes so far as to affirm that no sensible current whatever can be due to that cause, and adduces the authority of Sir John Herschel in support of that opinion:—"The doctrine of Captain Maury," he says, "was powerfully and convincingly opposed by Sir John Herschel; who showed, beyond all reasonable doubt, first, that the Gulf-stream really has its origin in the propulsive force of the trade-winds, and, secondly, that the greatest disturbance of equilibrium which can be supposed to result from the agencies invoked by Captain Maury would be utterly inadequate to generate and maintain either the Gulf-stream or any other sensible current" (§ 92). This being Dr. Carpenter's belief, it is somewhat singular that he should advance the case of the polar current passing under the Gulf-stream as evidence in favour of his theory; for in

reality he could hardly have selected a case more hostile to that theory. In short it is evident that, if a polar current impelled by a force other than that of gravity can pass from the banks of Newfoundland to the Gulf of Mexico (a distance of some thousands of miles) under a current flowing in the opposite direction and, at the same time, so powerful as the Gulf-stream, it could pass much more easily under comparatively still water, or water flowing in the same direction as itself. And if this be so, then all our difficulties disappear, and we satisfactorily explain the presence of cold polar water at the bottom of intertropical seas without having recourse to the hypothesis advanced by Dr. Carpenter.

But we have an example of an under current more inexplicable on the gravitation hypothesis than even that of the polar current, viz. the warm under current of Davis Strait.

There is a strong current flowing north from the Atlantic through Davis Strait into the Arctic Ocean underneath a surface-current passing southwards in an opposite direction. Large icebergs have been seen to be carried northwards by this under current at the rate of four knots an hour against both the wind and the surface-current, ripping and tearing their way with terrific force through surface-ice of great thickness. (See *Physical Geography of the Sea*, chap. ix. new edition, and Dr. A. Mühry 'On Ocean-currents in the Circumpolar Basin of the N. Hemisphere.') A current so powerful and rapid as this cannot, as Dr. Carpenter admits, be referred to difference of specific gravity. But even supposing that it could, still difference of temperature between the equatorial and polar sea would not account for it; for the current in question flows in the *wrong direction*. Nor will it help the matter the least to adopt Maury's explanation, viz. that the warm under current from the south, in consequence of its greater saltness, is denser than the cold one from the polar regions. For if the water of the Atlantic, notwithstanding its higher temperature, is in consequence of its greater saltness so much denser than the polar water on the west of Greenland as to produce an under current of four knots an hour in the direction of the pole, then surely the same thing to a certain extent will hold true in reference to the ocean on the east side of Greenland. Thus instead of there being, as Dr. Carpenter supposes, an underflow of polar water south into the Atlantic in virtue of its *greater density*, there ought, on the contrary, to be a surface-flow in consequence of its lesser density.

The true explanation no doubt is, that the warm under current from the south and the cold upper current from the north are both parts of one grand system of circulation produced by the winds, difference of specific gravity having no share whatever

either in impelling the currents, or in determining which shall be the upper and which the lower.

The wind in Baffin's Bay and Davis Strait blows nearly always in one direction, viz. from the north. The tendency of this is to produce a surface- or upper current from the north down into the Atlantic, and to prevent or retard any surface-current from the south. The warm current from the Atlantic, taking the path of least resistance, dips under the polar current and pursues its course as an under current.

Mr. Clement Markham, in his 'Threshold of the Unknown Region,' is inclined to attribute the motion of the icebergs to tidal action or to counter undercurrents. That the motion of the icebergs cannot reasonably be attributed to the tides is, I think, evident from the descriptions given both by Midshipman Griffin and by Captain Duncan, who distinctly saw the icebergs moving at the rate of about four knots an hour against a surface-current flowing southwards. And Captain Duncan states that the bergs continued their course northwards for several days, till they ultimately disappeared. The probability is that this northward current is composed partly of Gulf-stream water and partly of that portion of polar water which is supposed to flow round Cape Farewell from the east coast of Greenland. This stream, composed of both warm and cold water, on reaching to about latitude 65° N., where it encounters the strong northerly winds, dips down under the polar current and continues its northward course as an under current.

We have on the west of Spitzbergen, as has already been noticed, a similar example of a warm current from the south passing under a polar current. A portion of the Gulf-stream which passes round the west coast of Spitzbergen flows under an Arctic current coming down from the north; and it does so no doubt because it is here in the region of prevailing northerly winds, which favour the polar current but oppose the Gulf-stream. Again, we have a cold and rapid current sweeping round the east and south of Spitzbergen, a current of which Mr. Lamont asserts that he is positive he has seen it running at the rate of seven or eight miles an hour. This current, on meeting the Gulf-stream about the northern entrance to the German Ocean, dips down under that stream and pursues its course southwards as an under current.

Several other cases of under currents might be adduced which cannot be explained on the gravitation theory, and which must be referred to a system of oceanic circulation produced by the impulse of the wind; but these will suffice to show that the assumption that the winds can produce only a mere surface-drift is directly opposed to facts. And it will not do to affirm that a

current which forms part of a general system of circulation produced by the impulse of the winds cannot possibly be an under current; for in the case referred to we have proof that the thing is not only possible but actually exists. This point, however, will be better understood after we have considered the evidence in favour of a general system of oceanic currents.

Much of the difficulty experienced in comprehending how under currents can be produced by the wind, or how an impulse imparted to the surface of the ocean can ever be transmitted to the bottom, appears to me to result, to a considerable extent at least, from a slight deception of the imagination. The thing which impresses us most forcibly in regard to the ocean is its profound depth. A mean depth of, say, three miles produces a striking impression; but if we could represent to the mind the vast area of the ocean as correctly as we can its depth, *shallowness* rather than *depth* would be the impression produced. If in crossing a meadow we found a sheet of water one hundred yards in diameter and only an inch in depth, we should not call that a *deep* pool, we should call it a very *shallow* pool. The probability is that we should speak of it as simply a piece of ground covered with a thin layer of water. Yet such a thin layer of water would be a correct representation in miniature of the ocean; for the ocean in relation to its superficial area is as shallow as the pool of our illustration. In reference to such a pool or thin film of water, we have no difficulty in conceiving how a disturbance on its surface would be transmitted to its bottom. In fact our difficulty is in conceiving how any disturbance extending over its entire surface should not extend to the bottom. Now if we could form as accurate a sensuous impression of the vast area of the ocean as we do of such a pool, all our difficulty in understanding how the impulses of the wind acting on the vast area of the ocean should communicate motion down to its bottom would disappear.

The known condition of the ocean inconsistent with Dr. Carpenter's hypothesis.—Dr. Carpenter says that he looks forward with great satisfaction to the results of the inquiries which are being prosecuted by the Circumnavigation Expedition, in the hope that the facts brought to light may establish his theory of a general oceanic circulation; and he specifies certain of these facts which, if found to be correct, will establish his theory. It seems to me, however, that the facts to which he refers are just as explicable on the theory of under currents as on the theory of a general oceanic circulation. He begins by saying, "If the views I have propounded be correct, it may be expected that near the border of the great Antarctic ice-barrier a temperature below 30° will be met with (as it has been by Parry, Martens,

and Weyprecht near Spitzbergen) at no great depth beneath the surface, and that instead of rising at still greater depths, the thermometer will fall to near the freezing-point of salt water" (§ 39).

Dr. Carpenter can hardly claim this as evidence in favour of his theory; for near the borders of the ice-barrier the water, as a matter of course, could not be expected to have a much higher temperature than the ice itself. And if the observations be made during summer months, the temperature of the water at the surface will no doubt be found to be higher than that of the bottom; but if they be carried on during winter, the surface-temperature will doubtless be found to be as low as the bottom-temperature. These are results which do not depend upon any particular theory of oceanic circulation.

"The bottom-temperature of the North Pacific," he continues, "will afford a crucial test of the truth of the doctrine. For since the sole communication of this vast oceanic area with the Arctic basin is a strait so shallow as only to permit an inflow of warm surface-water, its deep cold stratum must be entirely derived from the Antarctic area; and if its bottom-temperature is not actually higher than that of the South Pacific, the glacial stratum ought to be found at a greater depth north of the equator than south of it" (§ 39).

This may probably show that the water came from the Antarctic regions, but cannot possibly prove that it came in the manner which he supposes.

"In the North Atlantic, again, the comparative limitation of communication with the Arctic area may be expected to prevent its bottom-temperature from being reduced as low as that of the Southern Atlantic" (§ 39). Supposing the bottom-temperature of the South Atlantic should be found to be lower than the bottom-temperature of the North Atlantic, this fact will be just as consistent with the theory of under currents as with his theory of a general movement of the ocean. Indeed I fear that even although Dr. Carpenter's expectations should eventually be realized in the results of the Circumnavigation Expedition, yet the advocates of the wind theory will still remain unconverted. In fact the Director of this Expedition has already, on the wind theory, offered an explanation of nearly all the phenomena on which Dr. Carpenter relies*; and the same has also been done by Dr. Petermann†, who, as is well known, is equally opposed to Dr. Carpenter's theory. Dr. Carpenter directs attention to the necessity of examining the broad and deep channel separating

* "Depths of the Sea." 'Nature' for July 28, 1870.

† "Mémorial on the Gulf-stream," *Geographische Mittheilungen* for vol. xvi. (1870).

Ice-land from Greenland. The observations which have already been made, however, show that nearly the entire channel is occupied, on the surface at least, by water flowing southward from the polar area—a direction the opposite of what it ought to be according to the gravitation theory. In fact the surface of one half of the entire area of the ocean, extending from Greenland to the North Cape, is moving in a direction the opposite of that which it ought to take according to the theory under review. The western half of this area is occupied by water which at the surface is flowing southwards; while the eastern half, which has hitherto been regarded by almost everybody but Dr. Carpenter himself and Mr. Findlay as an extension of the Gulf-stream, is moving polewards. The motion of the western half must be attributed to the winds and not to gravity; for it is moving in the wrong direction to be accounted for by the latter cause; but had it been moving in the opposite direction, no doubt its motion would have been referred to gravitation. To this cause the motion of the eastern half, which is in the proper direction, is attributed*; but why not assign this motion also to the impulse of the winds, more especially since the direction of the prevailing winds blowing over that area coincides with that of the water? If the wind can produce the motion of the water in the western half, why may it not do the same in the eastern half?

If there be such a difference of density between the equatorial and polar water as to produce a general flow of the upper portion of the ocean poleward, how does it happen that one half of the water in the above area is moving in opposition to gravity? How is it that in a wide open sea gravitation should act so powerfully in the one half of it and with so little effect in the other half? There is probably little doubt that the ice-cold water of the western half extends from the surface down to the bottom. And it is also probable that the bottom-water is moving southwards in the same direction as the surface-water. The bottom-water in such a case would be moving in harmony with the gravitation theory; but would Dr. Carpenter on this account attribute its motion to gravity? Would he attribute the motion of the lower half to gravity and the upper half to the wind? He could not in consistency with his theory attribute the motion of the upper half to gravity; for although the ice-cold water extended to the surface, this could not explain how gravity should move it southward instead of polewards, as according to theory it ought to move. He might affirm, if he chose, that the surface-water moves southwards because it is dragged forward by the bottom-water; but if this view be held, he is not entitled to

* Dr. Carpenter "On the Gulf-stream," *Proc. of Roy. Geog. Soc.* for January 9, 1871, § 29.

affirm, as he does, that the winds can only produce a mere surface-drift. If the viscosity and molecular resistance of water be such that, when the lower strata of the ocean are impelled forward by gravity or by any other cause, the superincumbent strata extending to the surface are perforce dragged after them, then, for the same reason, when the upper strata are impelled forward by the wind or any other cause, the underlying strata must also be dragged along after them.

If the condition of the ocean between Greenland and the north-western shore of Europe is irreconcilable with the gravitation theory, we find the case even worse for that theory when we direct our attention to the condition of the ocean on the southern hemisphere; for according to the researches of Captain Duperrey and others on the currents of the Southern Ocean, a very large portion of the area of that ocean is occupied by water moving on the surface more in a northward than a poleward direction. Referring to the deep trough between the Shetland and the Faroe Islands, called by him the "Lightning Channel," Dr. Carpenter says, "If my view be correct, a current-drag suspended in the *upper* stratum ought to have a perceptible movement in the N.E. direction; whilst another, suspended in the *lower* stratum, should move S.W." (§ 40).

Any one believing in the north-eastern extension of the Gulf-stream and in the Spitzbergen polar under current, to which I have already referred, would not feel surprised to learn that the surface-strata have a perceptible north-eastward motion, and the bottom strata a perceptible south-westward motion. North-east and east of Iceland there is a general flow of cold polar water in a south-east direction towards the left edge of the Gulf-stream. This water, as Professor Mohn concludes, "descends beneath the Gulf-stream and partially finds an outlet in the lower half of the Faroe-Shetland channel"*.

The Mechanics of the Theory.

"I now proceed," says Dr. Carpenter, "to the second head of the discussion, viz. the demonstration which Mr. Croll considers himself to have given, that the difference of temperature between polar and equatorial water cannot possibly produce the effect I attribute to it" (§ 21).

"Mr. Croll's whole manner of treating the subject is so different from that which it appears to me to require, and he has so completely misapprehended my own view of the question, that I feel it requisite to present this in fuller detail, in order that physicists and mathematicians, having both sides fully before them, may judge between us" (§ 26).

* Dr. Petermann's *Mittheilungen* for 1872, p. 315.

Dr. Carpenter then refers to a point so obvious as hardly to require consideration, viz. the effect which results when the surface of the entire area of a lake or pond of water is cooled. The whole of the surface-film being chilled at the same time, sinks through the subjacent water, and a new film from the warmer layer immediately beneath the surface rises into its place. This being cooled in its turn, sinks, and so on. He next considers what takes place when only a portion of the surface of the pond is cooled, and shows that in this case the surface-film which descends is replaced not from beneath, but by an inflow from the neighbouring area.

"That such must be the case," says Dr. Carpenter, "appears to me so self-evident that I am surprised that any person conversant with the principles of physical science should hesitate in admitting it, still more that he should explicitly deny it. But since others may feel the same difficulty as Mr. Croll, it may be worth while for me to present the case in a form of yet more elementary simplicity" (§ 29).

Then, in order to show the mode in which the general oceanic circulation takes place, he supposes two cylindrical vessels, W and C, of equal size to be filled with sea-water. Cylinder W represents the equatorial column, and the water contained in it has its temperature maintained at 60° ; whilst the water in the other cylinder C, representing the polar column, has its temperature maintained at 30° by means of the constant application of cold at the top. Free communication is maintained between the two cylinders at top and bottom; and the water in the cold cylinder being, in virtue of its low temperature, denser than the water in the warm cylinder, the two columns are therefore not in static equilibrium. The cold, and hence heavier column tends to produce an outflow of water from its bottom to the bottom of the warm column, which outflow is replaced by an inflow from the top of the warm column to the top of the cold column. In fact we have just a simple repetition of what he has given over and over again in his various memoirs on the subject. But why so repeatedly enter into the *modus operandi* of the matter? Who feels any difficulty in understanding how the circulation is produced?

Polar Cold considered by Dr. Carpenter the primum mobile.—It is evident that Dr. Carpenter believes that he has found in polar cold an agency the potency of which, in producing a general oceanic circulation, has been overlooked by physicists; and it is with the view of developing his ideas on this subject that he has entered so fully and so frequently into the exposition of his theory. "If I have myself done any thing," he says, "to strengthen the doctrine, it has been by showing that

polar cold, rather than equatorial heat, is the *primum mobile* of this circulation”*.

The influence of the sun in heating the waters of the intertropical seas is, in Dr. Carpenter’s manner of viewing the problem, of no great importance. The efficient cause of motion he considers resides in *cold* rather than in *heat*. In fact he even goes the length of maintaining that, as a power in the production of the general interchange of equatorial and polar water, the effect of polar cold is so much superior to that of intertropical heat, that the influence of the latter may be *practically disregarded*.

“Suppose two basins of ocean-water,” he says, “connected by a strait to be placed under such different climatic conditions that the surface of one is exposed to the heating influence of tropical sunshine, whilst the surface of the other is subjected to the extreme cold of the sunless polar winter. The effect of the surface-heat upon the water of the tropical basin will be for the most part limited (as I shall presently show) to its uppermost stratum, and may here be *practically disregarded*†.

Dr. Carpenter’s idea regarding the efficiency of cold in producing motion seems to me to be not only opposed to the generally received views on the subject, but wholly irreconcilable with the ordinary principles of mechanics. In fact there are so many points on which Dr. Carpenter’s theory of a “General Vertical Oceanic Circulation” differs from the generally received views on the subject of circulation by means of difference of specific gravity, that I have thought it advisable to enter somewhat minutely into the consideration of the mechanics of that theory, the more so as he has so repeatedly asserted that eminent physicists agree with what he has advanced on the subject.

According to the generally received theory, the circulation is due to the *difference of density* between the sea in equatorial and polar regions. The real efficient cause is gravity; but gravity cannot act when there is no difference of specific gravity. If the sea were of equal density from the poles to the equator, gravity could exercise no influence in the production of circulation; and the influence which it does possess is in proportion to the difference of density. But the difference of density between equatorial and polar waters is in turn due not absolutely either to polar cold or to tropical heat, but to both—or, in other words, to the *difference of temperature* between the polar and equatorial seas. This difference, in the very nature of things, must be as much the result of equatorial heat as of polar cold. If the sea in equatorial regions were not being heated by the sun as rapidly as the sea in polar regions is being cooled, the difference of tempe-

* Proc. Roy. Geog. Soc. January 9, 1871.

† Ibid.

perature between them, and consequently the difference of density, would be diminishing, and in course of time would disappear altogether. As has already been shown, it is a necessary consequence that the water flowing from equatorial to polar regions must be compensated by an equal amount flowing from polar to equatorial regions. Now, if the water flowing from polar to equatorial regions were not being heated as rapidly as the water flowing from equatorial to polar regions is being cooled, the equatorial seas would gradually become colder and colder until no sensible difference of temperature existed between them and the polar oceans. In fact, *equality of the two rates* is necessary to the very existence of such a general circulation as that advocated by Dr. Carpenter. If he admits that the general interchange of equatorial and polar water advocated by him is caused by the difference of density between the water at the equator and the poles, resulting from difference of temperature, then he must admit also that this difference of density is just as much due to the heating of the equatorial water by the sun as it is to the cooling of the polar water by radiation and other means—or, in other words, that it is as much due to equatorial heat as to polar cold. And if so, it cannot be true that polar cold rather than equatorial heat is the “*primum mobile*” of this circulation; and far less can it be true that the heating of the equatorial water by the sun is of so little importance that it may be “practically disregarded.”

Supposed influence of Heat derived from the Earth's Crust.—

There is, according to Dr. Carpenter, another agent concerned in the production of the general oceanic circulation, viz. the heat derived by the bottom of the ocean from the crust of the earth (see §§ 20, 34⁵; also Brit. Assoc. Report for 1872, p. 49, and other places). We have no reason to believe that the quantity of internal heat coming through the earth's crust is greater in one part of the globe than in another; nor have we any grounds for concluding that the bottom of intertropical seas receives more heat from the earth's crust than the bottom of those in polar regions. But if the polar seas receive as much heat from this source as the seas within the tropics, then the difference of density between the two cannot possibly be due to heat received from the earth's crust; and this being so, it is mechanically impossible that internal heat can be a cause in the production of the general oceanic circulation.

*Circulation without Difference of Level.—*There is another part of the theory which appears to me irreconcilable with mechanics. It is maintained that this general circulation takes place without any difference of level between the equator and the poles. Referring to the case of the two cylinders W and C, which re-

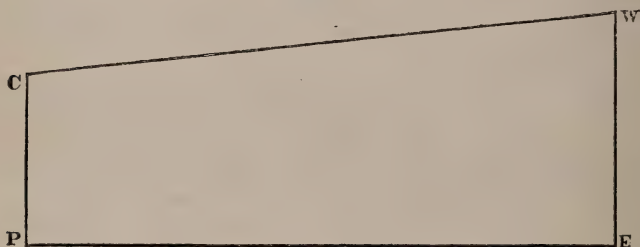
present the equatorial and polar columns respectively, Dr. Carpenter says:—

“The force which will thus lift up the entire column of water in W is that which causes the descent of the entire column in C, namely the excess of gravity constantly acting in C,—the levels of the two columns, and consequently their heights, being maintained at a *constant equality* by the free passage of surface-water from W to C.”

“The whole of Mr. Croll’s discussion of this question, however,” he continues, “proceeds upon the assumption that the levels of the polar and equatorial columns are *not kept at an equality, &c.*” (§ 30). And again, “Now, so far from asserting (as Captain Maury has done) that the trifling difference of level arising from inequality of temperature is adequate to the production of ocean-currents, I simply affirm that as fast as the level is disturbed by change of temperature it will be restored by gravity” (§ 23). See also to the same effect Brit. Assoc. Report, 1872, p. 50.

In order to understand more clearly how the circulation under consideration cannot take place without a difference of level, let W E (fig. 1) represent the equatorial column, and C P the

Fig. 1.



polar column. The equatorial column is warmer than the polar column because it receives *more* heat from the sun than the latter; and the polar is colder than the equatorial column because it receives *less* heat from the sun than the latter. The difference in the density of the two columns results from their difference of temperature; and the difference of temperature results in turn from the difference in the quantity of heat received from the sun by each. Or, to express the matter in other words, the difference of density (and consequently the circulation under consideration) is due to the excess of heat received from the sun by the equatorial over that received by the polar column; so that to leave out of account the superheating of the intertropical waters by the sun is to leave out of account the very thing of all others that is absolutely essential to the exist-

ence of the circulation. The water being assumed to be the same in both columns and differing only as regards temperature, and the equatorial column possessing more heat than the polar, and being therefore less dense than the latter, it follows, in order that the two columns may be in static equilibrium, that the surface of the equatorial column must stand at a higher level than that of the polar. This produces the slope WC from the equator to the pole. The extent of the slope will of course depend upon the extent of the difference of their temperatures. But, as was shown on a former occasion (*Phil. Mag.* for Oct. 1871), it is impossible that static equilibrium can ever be fully obtained, because the slope occasioned by the elevation of the equatorial column above the polar produces what we may be allowed to call a *molecular* disturbance of equilibrium. The surface of the ocean, or the molecules of water lying on the slope, are not in a position of equilibrium, but tend, in virtue of gravity, to roll down the slope in the direction of the polar column C. It will be observed that the more we gain of static equilibrium of the entire ocean the greater is the slope, and consequently the greater is the disturbance of molecular equilibrium; and, *vice versâ*, the more molecular equilibrium is restored by the reduction of the slope, the greater is the disturbance of static equilibrium. *It is therefore absolutely impossible that both conditions of equilibrium can be fulfilled at the same time so long as a difference of temperature exists between the two columns.* And this conclusion holds true even though we should assume water to be a perfect fluid absolutely devoid of viscosity. It follows, therefore, that a general oceanic circulation without a difference of level is a *mechanical impossibility*.

In a case of actual circulation due to difference of gravity, there is always a constant disturbance of both *static* and *molecular* equilibrium. Column C is always higher and column W always lower than it ought to be were the two in equilibrium; but they never can be at the same level.

It is quite conceivable, of course, that the two conditions of equilibrium may be fulfilled alternately. We can conceive column C remaining stationary till the water flowing from column W has restored the level. And after the level is restored we can conceive the polar column C sinking and the equatorial column W rising till the two perfectly balance each other. Such a mode of circulation, consisting of an alternate surface-flow and vertical descent and ascent of the columns, though conceivable, is in reality impossible in nature; for there are no means by which the polar column C could be supported from sinking till the level had been restored. But Dr. Carpenter does not assume that the general oceanic circulation takes

place in this intermitting manner; according to him, the circulation is *constant*. He asserts that there is a "*continual* transference of water from the bottom of C to the bottom of W, and from the top of W to the top of C, with a *constant* descending movement in C and a *constant* ascending movement in W" (§ 29). But such a condition of things is irreconcilable with the idea of "the levels of the two columns, and consequently their heights, being maintained at a *constant* equality" (§ 29).

Although Dr. Carpenter does not admit the existence of a permanent difference of level between the equator and the pole, he nevertheless speaks of a depression of level in the polar basin resulting from the contraction by cooling of the water flowing into it. This reduction of level induces an inflow of water from the surrounding area; "and since what is drawn away," to quote his own words, "is supplied from a yet greater distance, the continued cooling of the surface-stratum in the polar basin will cause a 'set' of waters towards it, to be propagated backwards through the whole intervening ocean in communication with it until it reaches the tropical area." The slope produced between the polar basin and the surrounding area, if sufficiently great, will enable the water in the surrounding area to flow polewards; but unless this slope extend to the equator, it will not enable the tropical waters also to flow polewards. One of two things necessarily follows: either the slope extends from the equator to the pole, or water can flow from the equator to the pole without a slope. If Dr. Carpenter maintains the former, he contradicts himself; and if he adopts the latter, he contradicts an obvious principle of mechanics.

A confusion of ideas in reference to the supposed agency of Polar Cold.—It seems to me that Dr. Carpenter has been somewhat misled by a slight confusion of ideas in reference to the supposed agency of polar cold. This is brought out forcibly in the following passage from his memoir in the 'Proceedings of the Royal Geographical Society,' vol. xv. p. 54.

"Mr. Croll, in arguing against the doctrine of a general oceanic circulation sustained by difference of temperature, and *justly maintaining* that such a circulation cannot be produced by the application of heat at the surface, has entirely ignored the agency of cold."

It is here supposed that there are two agents at work in the production of the general oceanic circulation. The one agent is *heat*, acting at the equatorial regions; and the other agent is *cold*, acting at the polar regions. It is supposed that the agency of cold is far more powerful than that of heat. In fact so trifling is the agency of equatorial heat in comparison with that of polar cold that it may be "practically disregarded"—left out

of account altogether,—polar cold being the *primum mobile* of the circulation. It is supposed also that I have considered the efficiency of one of the agents, viz. heat, and found it totally inadequate to produce the circulation in question; and it is admitted also that my conclusions are perfectly correct. But then I am supposed to have left out of account the other agent, viz. polar cold, the only agent possessing real potency. Had I taken into account polar cold, it is supposed that I should have found at once a cause perfectly adequate to produce the required effect.

This is a fair statement of Dr. Carpenter's views on the subject; I am unable, at least, to attach any other meaning to his words. And I have no doubt they are also the views which have been adopted by those who have accepted his theory.

It must be sufficiently evident from what has already been stated, that the notion of there being two separate agents at work producing circulation, namely heat and cold, the one of which is assumed to have much more potency than the other, is not only opposed to the views entertained by physicists, but is also wholly irreconcilable with the ordinary principles of mechanics. But more than this, if we analyze the subject a little so as to remove some of the confusion of ideas which besets it, we shall find that these views are irreconcilable with even Dr. Carpenter's own explanation of the cause of the general oceanic circulation.

Cold and *heat*, considered as sensations, are very different things; but cold considered as a condition of a body means only a deficiency or absence of heat. When we say, for example, that the polar seas are colder than the equatorial, our meaning is that the polar seas possess less heat than the equatorial. And when we say that the equatorial seas are hotter than the polar, our meaning of course likewise is that the equatorial seas possess more heat than the polar. Or if we say that the equatorial seas are *hot* and the polar seas *cold*, we mean simply that both seas possess a certain amount of heat, the equatorial seas having more than the polar; or, judging them by our sensations, we call the one *hot* and the other *cold*.

How, then, according to Dr. Carpenter, does polar cold impart motion to the water? The warm water flowing in upon the polar column becomes chilled by cold, but it is not cooled below that of the water underneath; for, according to Dr. Carpenter, the ocean in polar regions is as cold and as dense underneath as at the surface. The cooled surface-water does not sink through the water underneath, like the surface-water of a pond chilled during a frosty night. "The descending motion in column C will not consist," he says, "in a successional descent

of surface-films from above downwards, but it will be a downward movement of the *entire mass*, as if water in a tall jar were being drawn off through an orifice at the bottom" (§ 29). There is a downward motion of the entire column, producing an outflow of water at the bottom towards the equatorial column W, which outflow is compensated by an inflow from the top of the equatorial column to the top of the polar column C. But what causes column C to descend? The cause of the descent is its excess of weight over that of column W. Column C descends and column W ascends, for the same reason that in a balance the heavy scale descends and the light scale rises. Column C descends not simply because it is cold, but because it is *colder* than column W. Column C descends not simply because in consequence of being cold it is dense and therefore heavy, but because in consequence of being cold it is *denser* and therefore *heavier* than column W. It might be as cold as frozen mercury and as heavy as lead; but it would not on that account descend unless it were heavier than column W. The descent of column C and ascent of column W, and consequently the general oceanic circulation, results, therefore, according to Dr. Carpenter's explanation, from the *difference* in the weights of the two columns; and the difference in the weights of the two columns results from their *difference* of density; and the difference of density of the two columns in turn results from their *difference* of temperature. But it has already been proved that the difference of temperature between the polar and equatorial columns depends wholly on the difference in the amount of heat received by each from the sun. The equatorial column W possesses more heat than the polar column C, solely because it receives more heat from the sun than column C. Consequently Dr. Carpenter's statement that the circulation is produced by polar cold rather than by equatorial heat, is just as much in contradiction to his own theory as it is to the principles of mechanics. Again, his admission that the general oceanic circulation "cannot be produced by the application of heat to the surface," is virtually a giving up the whole point in debate; for according to his gravitation theory, and every form of that theory, the circulation results from *difference* of temperature between equatorial and polar seas; but this difference, as we have seen, is entirely owing to the difference in the amount of heat received from the sun at these two places. The heat received, however, is "surface-heat;" for it is at the surface that the ocean receives all its heat from the sun; and consequently if surface-heat cannot produce the effect required, nothing else can.

M. Dubuat's *experiments*.—Referring to the experiments of M. Dubuat adduced by me to show that water would not run

down a slope of 1 in 1,820,000*, he says, "Now the experiments of M. Dubuat had reference, not to the slow restoration of level produced by the motion of water on itself, but to the sensible movement of water flowing over solid surfaces and retarded by its friction against them" (§ 22). Dr. Carpenter's meaning, I presume, is that if the incline consist of any solid substance, water will not flow down it; but if it be made of *water* itself, *water* will flow down it. But in M. Dubuat's experiments it was only the molecules in actual *contact* with the solid incline that could possibly be retarded by friction against it. The molecules not in contact with the solid incline evidently rested upon an *incline of water*, and were at perfect liberty to roll down that incline if they chose; but they did not do so; and consequently M. Dubuat's experiment proved that water will not flow over itself on an incline of 1 in 1,000,000.

A begging of the question at issue.—"It is to be remembered," says Dr. Carpenter, "that, however small the original amount of movement may be, a *momentum* tending to its continuance *must* be generated from the instant of its commencement; so that if the initiating force be in constant action, there will be a *progressive acceleration* of its rate, until the increase of resistance equalizes the tendency to further acceleration. Now, if it be admitted that the propagation of the disturbance of equilibrium from one column to another is simply *retarded*, *not* prevented, by the viscosity of the liquid, I cannot see how the conclusion can be resisted, that the constantly maintained difference of gravity between the polar and equatorial columns really acts as a *vis viva* in maintaining a circulation between them" (§ 35).

If it be true, as Dr. Carpenter asserts, that in the case of the general oceanic circulation advocated by him "viscosity" simply *retards* motion, but does not *prevent* it, I certainly agree with him "that the constantly maintained difference of gravity between the polar and equatorial columns really acts as a *vis viva* in maintaining a circulation between them." But to assert that it merely retards, but does not prevent, motion, is simply *begging the question at issue*. It is an established principle that if the *force* resisting motion be greater than the force tending to produce it, then no motion can take place and no work can be performed. The experiments of M. Dubuat prove that the *force* of the molecular resistance of water to motion is *greater* than the *force* derived from a slope of 1 in 1,000,000; and therefore it is simply begging the question at issue to assert that it is *less*. The experiments of MM. Barlow, Rainey, and others to which

* The slope, however, taking Dr. Carpenter's own data, amounts only to little more than one half, viz. to 1 in 3,500,000. See Phil. Mag. for October 1871, p. 263.

he alludes, are scarcely worthy of consideration in relation to the present question, because we know nothing whatever regarding the actual amount of force producing motion of the water in these experiments, further than that it must have been enormously greater than that derived from a slope of 1 in 1,000,000.

Supposed argument from the tides.—Dr. Carpenter advances Mr. Ferrel's argument in regard to the tides. The power of the moon to disturb the earth's water, he asserts, is, according to Herschel, only one 11,400,000th part of gravity, and that of the sun not over one 25,736,400th part of gravity; yet the moon's attractive force, even when counteracted by the sun, will produce a rise of the ocean. But as the disturbance of gravity produced by difference of temperature is far greater than the above, it ought to produce circulation.

It is here supposed that the force exerted by gravity on the ocean, resulting from difference of temperature, tending to produce the general oceanic circulation, is much greater than the force exerted on the ocean by the moon in the production of the tides. But if we examine the subject we shall find that the opposite is the case. The attraction of the moon tending to lift the waters of the ocean acts directly on every molecule from the surface to the bottom; but the force of gravity tending to produce the circulation in question acts directly on only a portion of the ocean. Gravity can exercise no direct force in impelling the underflow from the polar to the equatorial regions, nor in raising the water to the surface when it reaches the equatorial regions. Gravity can exercise no direct influence in pulling the water horizontally along the earth's surface, nor in raising it up to the surface. The pull of gravity is always *downwards*, never *horizontally* nor upwards. Gravity will tend to pull the surface-water from the equator to the poles because here we have *descent*. Gravity will tend to sink the polar column because here also we have *descent*. But these are the only parts of the circuit where gravity has any tendency to produce motion. Motion in the other parts of the circuit, viz. along the bottom of the ocean from the poles to the equator and in raising the equatorial column, is produced by the *pressure* of the polar column; and consequently it is only *indirectly* that gravity may be said to produce motion in those parts. It is true that on certain portions of the ocean the force of gravity tending to produce motion is greater than the force of the moon's attraction, tending to produce the tides; but this portion of the ocean is of inconsiderable extent. The total force of gravity acting on the entire ocean tending to produce circulation is in reality prodigiously less than the total force of the moon tending to produce the tides.

It is no doubt a somewhat difficult problem to determine accurately the total amount of force exercised by gravity on the ocean; but for our present purpose this is not necessary. All that we require at present is a very rough estimate indeed. And this can be attained by very simple considerations. Suppose we assume the mean depth of the sea to be, say, three miles. The mean depth may yet be found to be somewhat less than this, or it may be found to be somewhat greater; a slight mistake, however, in regard to the mass of the ocean will not materially affect our conclusions. Taking the depth at 3 miles, the force or direct pull of gravity on the entire waters of the ocean tending to the production of the general circulation will not amount to more than $\frac{1}{12,000,000,000}$ that of gravity, or only about $\frac{1}{1053}$ that of the attraction of the moon in the production of the tides. Let it be observed that I am referring to the force or pull of gravity, and not to hydrostatic pressure.

The moon, by raising the waters of the ocean, will produce a slope of 2 feet in a quadrant; and because the raised water sinks and the level is restored, Mr. Ferrel concludes that a similar slope of 2 feet produced by difference of temperature will therefore be sufficient to produce motion and restore level. But it is overlooked that the restoration of level in the case of the tides is as truly the work of the moon as the disturbance of that level is. For the water raised by the attraction of the moon at one time is again, six hours afterwards, pulled down by the moon when the earth has turned round a quadrant.

No doubt the earth's gravity alone would in course of time restore the level; but this does not follow as a logical consequence from Mr. Ferrel's premises. If we suppose a slope to be produced in the ocean by the moon and the moon's attraction withdrawn so as to allow the water to sink to its original level, the raised side will be the heaviest and the depressed side the lightest; consequently the raised side will tend to sink and the depressed side will tend to rise, in order that the ocean may regain its static equilibrium. But when a difference of level is produced by difference of temperature, the raised side is always the lightest and the depressed side is always the heaviest; consequently the very effort which the ocean makes to maintain its equilibrium tends to prevent the level being restored. The moon produces the tides chiefly by means of a simple yielding of the entire ocean considered as a mass; whereas in the case of a general oceanic circulation the level is restored by a *flow* of water at or near the surface. Consequently the amount of friction and molecular resistance to be overcome in the restoration of level in the latter case is much greater than in the former. The moon, as the researches of Sir William Thomson

show, will produce a tide in a globe composed of a substance where no currents or general flow of the materials could possibly take place.

Pressure as a Cause of circulation.—We shall now briefly refer to the influence of pressure (the indirect effects of gravity) in the production of the circulation under consideration. That which causes the polar column C to descend and the equatorial column W to ascend, as has repeatedly been remarked, is the difference in the weight of the two columns. The efficient cause in the production of the movement is, properly speaking, gravity; *cold* at the poles and *heat* at the equator, or, what is the same thing, the *excess* of heat received by the equator over that received by the poles is what maintains the difference of temperature between the two columns, and consequently is that also which maintains the difference of weight between them. In other words, difference of temperature is the cause which maintains the *state of disturbed equilibrium*. But the efficient cause of the circulation in question is gravity. Gravity, however, could not act without this state of disturbed equilibrium; and difference of temperature may therefore be called, in relation to the circulation, a necessary *condition*, while gravity may be termed the *cause*. Gravity sinks column C *directly*, but it raises column W *indirectly* by means of pressure. The same holds true in regard to the motion of the bottom-waters from C to W, which is likewise due to pressure. The pressure of the excess of the weight of column C over that of column W impels the bottom-water equatorwards and lifts the equatorial column. But on this point I need not at present dwell, as I have in my last paper entered into a full discussion as to how this takes place*.

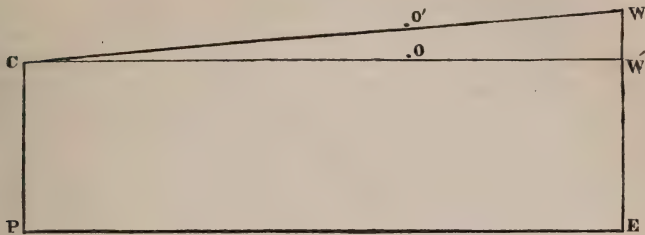
We come now to the most important part of the inquiry, viz. how is the surface-water impelled from the equator to the poles? Is pressure from behind the impelling force here as in the case of the bottom-water of the ocean? It seems to me that, in attempting to account for the surface-flow from the equator to the poles, Dr. Carpenter's theory signally fails. The force to which he appeals appears to be wholly inadequate to produce the required effect.

The experiments of M. Dubuat, as already noticed, prove that any slope which can possibly result from the difference of temperature between the equator and the poles is wholly insufficient to enable gravity to move the waters; but it does not necessarily prove that the *pressure* resulting from the raised water at the equator may not be sufficient to produce motion. This point will be better understood from the following figure, where, as

* Phil. Mag. for October 1871.

before, P C represents the polar column and E W the equatorial column.

Fig. 2.



It will be observed that the water in that wedge-shaped portion W C W' forming the incline cannot be in a state of static equilibrium. A molecule of water at O, for example, will be pressed more in the direction of C than in the direction of W', and the amount of this excess of pressure towards C will depend upon the height of W above the line C W'. It is evident that the pressure tending to move the molecule at O towards C will be far greater than the direct pull of gravity tending to draw a molecule at O' lying on the surface of the incline towards C. The experiments of M. Dubuat prove that the direct force of gravity will not move the molecule at O'—that is, cause it to roll down the incline W C; but they do not prove that it may not yield to pressure from above, or that the pressure of the column W W' will not move the molecule at O. The pressure is caused by gravity, and cannot, of course, enable gravity to perform more work than what is derived from the energy of gravity; it will enable gravity, however, to overcome resistance, which it could not do by direct action. But whether the pressure resulting from the greater height of the water at the equator due to its higher temperature be actually sufficient to produce displacement of the water is a question which I am wholly unable to answer.

If we suppose 9 feet to be the height of the equatorial surface above the polar required to make the two columns balance each other, the actual difference of level between the two columns will certainly not be more than one half that amount, because, if a circulation exist, the weight of the polar column must always be in excess of that of the equatorial. But this excess can only be obtained at the expense of the surface-slope, as was shown at length in my last paper. The surface-slope probably will not exceed more than 4 feet or $4\frac{1}{2}$ feet. Suppose the ocean to be of equal density from the poles to the equator, and that by some means or other the surface of the ocean at the equator is raised, say, 4 feet above that of the poles, then there can be little

doubt that in such a case the water would soon regain its level ; for the ocean at the equator being heavier than at the poles by the weight of a layer 4 feet in thickness, it would sink at the former place and rise at the latter until equilibrium was restored, producing, of course, a very slight displacement of the bottom-waters towards the poles. It will be observed, however, that restoration of level in this case takes place by a simple yielding, as it were, of the entire mass of the ocean without displacement of the molecules of the water over each other to any great extent. In the case of a slope produced by difference of temperature, however, the raised portion of the ocean is not heavier but lighter than the depressed portion, and consequently has no tendency to sink. Any movement which the ocean as a mass makes in order to regain equilibrium tends, as we have seen, rather to increase the difference of level than to reduce it. Restoration of level can only be produced by the forces which are in operation in the wedge-shaped mass WCW' , constituting the slope itself. But it will be observed by a glance at the figure that, in order to the restoration of level, a large portion of the water WW' at the equator will require to flow to C , the pole.

According to the general *vertical* oceanic circulation theory, pressure from behind is not one of the forces employed in the production of the flow from the equator to the poles. This is evident ; for there can be no pressure from behind acting on the water if there be no slope existing between the equator and the poles. Dr. Carpenter not only denies the actual existence of a slope, but denies the necessity for its existence. But to deny the existence of a slope is to deny the existence of pressure, and to deny the necessity for a slope is to deny the necessity for pressure. That in Dr. Carpenter's theory the surface-water is supposed to be *drawn* from the equator to the poles, and not *pressed* forward by a force from behind, is further evident from the fact that he maintains that the force employed is not *vis a tergo* but *vis a fronte* (Proc. Roy. Geog. Soc. Jan. 9, 1871, § 29).

[To be continued.]

XV. On Quartz, Ice, and Karstenite. By W. H. MILLER, M.A., F.R.S., Professor of Mineralogy in the University of Cambridge*.

Quartz.

AMONG the minerals presented to the University by H. W. Elphinstone, Esq., are two crystals of quartz associated with chlorite, apparently from the same, but unknown, locality.

* Communicated by the Author.

Each of these crystals exhibits one face of a rhombohedron, having angles which differ too widely from those of the forms described by Des Cloizeaux in his *Manuel de Minéralogie* to admit of identification with any of them, and therefore has probably never been observed before.

The larger of the two crystals, besides the supposed new face, which will be denoted by the letter ε , has the forms $2\bar{1}\bar{1}$, 100 , $\bar{1}22$, $8\bar{1}\bar{1}$, $10\bar{1}\bar{1}$, $\alpha 14\bar{2}$, $\alpha 4\bar{1}\bar{2}$. The faces ε , 100 are rather uneven, the bisection of the images of the bright signal being uncertain to the extent of about $2'$ in the former and rather less in the latter. Three observations of the angle between these faces gave $30^\circ 23'5$, $30^\circ 23'5$, $30^\circ 24'$ respectively.

The other crystal has the forms $2\bar{1}\bar{1}$, 100 , $\bar{1}22$, $\alpha 41\bar{2}$, $\alpha 4\bar{1}\bar{2}$ in addition to ε . This last face is very even and bright; but 100 is rather imperfect. The observed angle between these faces lies between $30^\circ 22'2$ and $30^\circ 28'4$.

Of the faces given by Des Cloizeaux, those which most nearly approach the position of ε make with 100 angles of $29^\circ 26'$, $30^\circ 4'$, $30^\circ 44'$, having for their symbols $11\bar{4}\bar{4}$, $8\bar{3}\bar{3}$, $13\bar{5}\bar{5}$ respectively. In order to obtain more probable values of the indices of ε , let us suppose the angle between 100 and ε to be $30^\circ 24'$, hkk the symbol of ε , and D , T the angles which the axis of the rhombohedron makes with normals to the faces 100 , hkk . Then, since $D=51^\circ 47'$ and $T-D=30^\circ 24'$, we have $T=82^\circ 11'$.

But

$$\frac{h-k}{h+2k} = \frac{\tan T}{\tan D} = 5.7356.$$

The converging fractions approximating to this number are:—

$$\frac{5}{1}, \frac{6}{1}, \frac{17}{3}, \frac{23}{4}, \frac{86}{15}, \dots$$

The first two fractions give the faces $11\bar{4}\bar{4}$, $13\bar{5}\bar{5}$ already noticed; the third a face $37\bar{14}\bar{14}$, making an angle of $30^\circ 18'$ with 100 , and therefore not very probable; the fourth a face making with 100 an angle of $30^\circ 25'$, which, taking into account the imperfections of the faces of the crystal, agrees sufficiently well with the observations. The resulting symbol is $50\bar{19}\bar{19}$.

The fraction $\frac{40}{7}$, obtained by adding the numerators and denominators of the third and fourth fractions, leads to the symbol $29\bar{11}\bar{11}$. The face of which this is the symbol makes with 100 an angle of $30^\circ 22'$, and is therefore hardly so probable as

50 $\overline{19} \overline{19}$, notwithstanding the lower values of the indices of the former symbol.

Ice.

In a memoir by Franz Leydolt, entitled "Beiträge zur Kenntniss der Krystallform und der Bildungsart des Eises," it is asserted that ice has no cleavage (*Sitzungsberichte der mathem.-naturw. Cl. der kais. Akad der Wissensch.* Band vii. Abth. 2, p. 477). A good many years previously I had seen some plates of ice broken which exhibited a separation parallel to the surfaces of the ice so perfectly like cleavage, that I never hesitated to publish the statement that ice has a cleavage parallel to the faces of the form 111 . A considerable time elapsed after the appearance of Leydolt's paper before an opportunity of making further observations presented itself. When at last I obtained some thick plates of newly formed ice, I was unable to procure a trace of cleavage by the application of knife, chisel, or point in a direction parallel to their bounding planes. On throwing one of the plates on the hard frozen ground it broke across, exhibiting in the fracture two planes normal to the natural faces of the plate, and apparently (for I had not at hand the means of measuring the angle they made with one another) parallel to two adjacent faces of a regular six-sided prism, looking like very perfect cleavages, and affording by reflection distinct images of surrounding objects; but I was unable to obtain a trace of cleavage in planes parallel to either of those revealed by fracture. It is therefore obvious that the separations, as well parallel as normal to the surfaces of the plates of ice, were due to the existence of faces of union and not to true cleavage. The latter planes are probably those of the six-sided prism $10\overline{1}$; for some crystals of ice examined by A. E. Nordenskjöld were combinations of the simple forms 111 , $\kappa 321$, $\kappa 210$, $\kappa 51\overline{3}$, $10\overline{1}$. The angles which normals to the faces of these forms make with a normal to 111 are approximately:—

$$111, 321 = 38^\circ 57'; \quad 111, 210 = 58^\circ 15';$$

$$111, 51\overline{3} = 81^\circ 31'; \quad 111, 10\overline{1} = 90^\circ.$$

(Poggendorff's *Annalen*, vol. cxiv. p. 612.)

Karstenite.

A small cavity in the interior of a mass of Karstenite (CaO SO^3) from Lüneburg was found to be traversed by several slender crystals attached at both ends to the walls of the cavity. These crystals exhibit some simple forms hitherto undescribed, and several of the forms first observed by Hessenberg and described by him in the 10th Number of his "Mineralogische Notizen,"

published in the *Abhandlungen der Senckenbergischen Naturforschenden Gesellschaft*, Frankfurt a. M. vol. viii.

Let the symbols 100, 010 denote faces normal to the lines bisecting the obtuse and acute angles between the optic axes, 001 a face parallel to the plane of the optic axes, and 110 the face which, according to the observations of Hessenberg, makes with 100 an angle of $48^{\circ} 15'$, the angle between two planes being measured by the angle between normals to them drawn from any point in the interior of the crystal.

The crystals from Lüneburg exhibited faces of the forms 100, 010, 110, 210, 310, 510, 540, 320, 410, 430, 150, 520, 530, of which the 4th... 9th were first observed by Hessenberg. The last four appear to be new. The faces of all these forms give very indistinct reflections, with a single exception: in one of the crystals a face of the form 110 was very perfect; the observed values of the angles it made with the cleavage $\bar{1}00$ were:— $131^{\circ} 45'$, $44' \cdot 2$, $44' \cdot 55$, $44' \cdot 5$, $44' \cdot 7$; mean $131^{\circ} 44' \cdot 6$. Hence $100, 110 = 48^{\circ} 15' \cdot 4$. The angles between the different faces and the face 100, taking Hessenberg's value of the angle 100, 110, are:—

010, 001 . . .	$90^{\circ} 0'$
100, 010 . . .	$90^{\circ} 0'$
100, 001 . . .	$90^{\circ} 0'$
100, 510 . . .	$12^{\circ} 38'$
100, 410 . . .	$15^{\circ} 39'$
100, 310 . . .	$20^{\circ} 29'$
100, 520 . . .	$24^{\circ} 8'$
100, 210 . . .	$29^{\circ} 15'$
100, 530 . . .	$33^{\circ} 55'$
100, 320 . . .	$36^{\circ} 45'$
100, 430 . . .	$40^{\circ} 2'$
100, 540 . . .	$41^{\circ} 52'$
100, 110 . . .	$48^{\circ} 15'$
100, 150 . . .	$79^{\circ} 53'$

When light is refracted through the prism bounded by the planes $\bar{1}00, 110$, the least-refracted ray is polarized in a plane parallel to 001. The indices of the light in this plane are:—

Extreme red	1.569
Brightest part of spectrum . . .	1.577
Boundary of green and blue . . .	1.580
Extreme violet	1.589

In water one optic axis seen through the faces 110, $\bar{1}\bar{1}0$.

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and the other seen through the faces $\bar{1}10$, $1\bar{1}0$, appeared to make with one another an angle of $36^\circ 22'$. Hence the direction of either optic axis within the crystal makes an angle of $22^\circ 1'$ with a normal to the face 010 .

In crystals from Berchtesgaden, Hesseberg searched in vain for a face of the form 101 , for the existence of which I considered that I had had satisfactory evidence. I therefore reexamined the crystal in which I supposed it to be visible. Using a spot of sunlight reflected from a plane mirror as the bright signal, the crystal being adjusted so that the intersection of the faces $1\bar{1}1$, 111 was parallel to the axis of the instrument, an image of the spot of sunlight was seen as if reflected from a face making equal angles with the faces $1\bar{1}1$, 111 ; but on making the crystal revolve round the axis of the branch of the holder parallel to the plane of the circle, the spot remained immovable. Hence it is evident that the spot of light seen was not due to a single reflection, but to a reflection at each of two separations in the interior of the crystal parallel to the faces 001 and 100 .

In several of the fragments of crystals from Berchtesgaden faint separations indicative of cleavages were observed, which, on measuring the angles they made with the face 100 with a position-micrometer, were found to be parallel to the faces of the form 110 . It was not found possible to separate the crystal in the direction of this cleavage, on account of the superior facility of the other cleavages.

Some colourless crystals from Stassfurth, given me by M. Pisani, of Paris, had the faces of many of the simple forms striated to such an extent that it was extremely difficult to measure the angles they made with one another. By using for the bright signal a large beam of sunlight reflected from the mirror of a heliostat, I think I have ascertained the existence of the following forms, the last two being the least certain:—

100 , 010 , 001 , 110 , 210 , 310 , 320 , 430 , 510 ,
 102 , 203 .

001 , 102 . . . $26^\circ 33'$
 001 , 203 . . . $33^\circ 40'$

XVI. *Measurements of the Polarization of the Light reflected by the Sky and by one or more Plates of Glass.* By Professor EDWARD C. PICKERING, *Boston, U.S.**

THE following observations, which will be published in full in the 'Proceedings of the American Academy of Arts and Sciences,' were conducted to test Fresnel's formula for the reflection of light. He showed that, if the light was polarized in the plane of incidence, the amount reflected would be $A = \frac{\sin^2(i-r)}{\sin^2(i+r)}$, while if polarized in a plane perpendicular to it the proportion would be $B = \frac{\tan^2(i-r)}{\tan^2(1+r)}$, i and r representing the angles of incidence and refraction respectively. Natural light may be regarded as composed of two equal beams polarized at right angles, hence the amount reflected

$$R = \frac{1}{2}(A + B) = \frac{1}{2} \left(\frac{\sin^2(i-r)}{\sin^2(i+r)} + \frac{\tan^2(i-r)}{\tan^2(i+r)} \right),$$

a formula which may be applied to any special case by substituting proper values for i and r . The value of A evidently increases as i varies from 0° to 90° . That of B , on the other hand, diminishes from 0° until $i+r=90^\circ$, when it equals 0; or at this angle, which is that of total polarization, all of the ray B is transmitted, all the reflected beam being polarized in the plane of incidence. When $i=90^\circ$, $A=1$, $B=1$; hence all the light is reflected. When $i=0^\circ$, A , B , and R equal $\left(\frac{n-1}{n+1}\right)^2$; hence the reflected light increases with n , being zero when $n=1$, and 100 per cent. when $n=\infty$. Many familiar phenomena are thus readily accounted for—for instance, the brightness of the diamond, the covering power of white lead as a paint, and the brilliancy of wet or varnished stones and woods.

A curious case presents itself when $n=1+dn$, or differs from unity only by an infinitesimal amount. A then becomes equal to $\frac{1}{4}dn^2(1+\tan^2 i)^2$, and B to $\frac{1}{4}dn^2(1-\tan^2 i)^2$. When $i=0$, A , B , and R equal $\frac{1}{4}dn^2$; and this quantity is accordingly taken as the unit in Table I. The first column gives various values of the angle of incidence, the second and third the corresponding values of A and B , the fourth the amount of light reflected, and the fifth the degree of polarization. The other columns will be explained hereafter. This Table is evidently applicable to all cases where the media bounding the surface have nearly the same index of refraction, whether its absolute amount is great or small.

* Communicated by the Author.

TABLE I.

Light reflected when n is near unity, or equals $1 + dn$.

i .	A.	B.	$\frac{1}{2}(A+B)$.	$\frac{A-B}{A+B}$.	Theoretical.	Observed.	Difference.
0	1.000	1.000	1.000	0.0	0.0	0.0	0.0
5	1.015	.985	1.000	1.5	1.0	1.0	0.0
10	1.063	.939	1.001	6.2	4.2	3.5	+0.7
15	1.149	.862	1.005	14.3	10.0	9.0	+1.0
20	1.282	.752	1.017	26.0	18.2	17.5	+0.7
25	1.482	.612	1.047	41.5	29.0	28.5	+0.5
30	1.778	.444	1.111	60.0	42.0	41.0	+1.0
35	2.221	.260	1.240	79.1	55.4	56.0	-0.6
40	2.904	.088	1.496	94.5	66.1	67.0	-0.9
45	4.000	.000	2.000	100.0	70.0	72.0	-2.0
50	5.857	.176	3.016	94.5	66.1	68.0	-1.9
55	9.239	1.081	5.160	79.1	55.4	58.0	-2.6
60	16.000	4.000	10.000	60.0	42.0	44.0	-2.0
65	31.346	12.952	22.149	41.5	29.0	30.0	-1.0
70	73.079	42.884	57.981	26.0	18.2	18.0	+0.2
75	222.85	167.16	195.00	14.3	10.0	8.5	+1.5
80	1099.85	971.21	1035.53	6.2	4.2	3.0	+1.2
85	17330.64	16808.08	17069.36	1.5	1.0	1.0	0.0
90	∞	∞	∞	0.0	0.0	0.0	0.0

The most important application of Fresnel's formula is to the case of glass, where n somewhat exceeds 1.5. The first portion of Table II. gives in an abbreviated form the result of a computation for various values of i , A , B , R , and the polarization of the reflected and refracted rays. When, as frequently happens in the case of plates of glass, the ray passes through several parallel surfaces, a portion of the light reflected back by the second surface is again intercepted by the first surface. It may readily be proved that, if A is the amount reflected by a single surface, the amount transmitted including this internal reflection will be

$\frac{1-A}{1+(m-1)A}$, while if no internal reflection took place it would be only $(1-A)^m$. In Table II. the values of A , B , R and of the polarization are given for 2, 8, and 20 surfaces, corresponding to 1, 4, and 10 plates of glass. In all these cases the index $n=1.55$.

TABLE II.—Light reflected by 1, 2, 8, and 20 surfaces.

1 surface.						2 surfaces, 1 plate.				
i.	A.	B.	Re- flec- ted.	Per cent. po- larization.		A.	B.	Re- flec- ted.	Per cent. po- larization.	
				Refl.	Refr.				Refl.	Refr.
0	4.6	4.6	4.6	0.0	0.0	8.8	8.8	8.8	0.0	0.0
5	4.7	4.6	4.6	1.0	0.0	9.0	8.7	8.8	1.0	0.1
10	4.8	4.5	4.7	4.0	0.2	9.2	8.6	8.9	4.1	0.4
15	5.1	4.2	4.7	9.1	0.4	9.8	8.1	8.9	9.2	0.9
20	5.4	3.9	4.7	16.4	0.8	10.4	7.5	8.9	16.3	1.6
25	5.9	3.5	4.7	25.9	1.3	11.1	6.7	8.9	25.4	2.5
30	6.6	3.0	4.8	37.8	1.9	12.5	5.8	9.1	36.4	3.6
35	7.5	2.4	5.0	51.7	2.7	14.2	3.7	9.4	49.3	5.2
40	8.8	1.7	5.3	66.7	3.7	16.4	3.5	10.0	64.0	7.2
45	10.4	1.1	5.7	81.2	4.9	19.0	2.1	10.6	80.1	9.4
50	12.5	0.5	6.5	92.9	6.5	22.4	0.5	11.4	95.6	12.5
55	15.4	0.1	7.7	99.3	8.3	26.9	0.1	14.0	99.3	15.6
57	17.0	0.0	8.5	100.0	9.3	29.1	0.0	14.5	100.0	17.0
60	19.3	0.1	9.7	98.8	10.6	32.4	0.2	16.3	98.8	19.2
65	24.7	1.1	12.9	91.2	13.5	38.4	2.5	20.4	87.7	23.2
70	32.0	4.0	18.0	77.7	17.1	48.5	7.7	28.1	72.6	28.4
75	42.0	10.4	26.2	61.8	21.9	59.6	19.7	39.1	52.2	33.2
80	55.7	23.3	39.5	41.0	26.8	71.7	38.0	54.8	30.7	37.2
82	62.6	31.6	47.1	32.8	29.3	77.1	48.3	62.7	22.7	38.6
84	70.3	42.4	56.3	24.7	32.1	82.7	60.2	71.4	15.7	39.6
85	74.5	49.0	61.8	20.6	33.3	85.4	68.5	75.8	12.6	40.0
86	79.0	56.6	67.2	16.5	34.8	88.2	72.5	80.3	9.8	40.4
87	83.8	65.3	74.6	12.4	36.3	91.4	79.2	85.3	7.0	40.8
88	88.9	75.3	82.1	8.2	37.9	94.1	86.1	90.1	4.4	41.0
89	94.3	86.8	90.5	4.1	39.6	96.9	92.9	94.9	2.0	41.1
90	100.0	100.0	100.0	0.0	41.2	100.0	100.0	100.0	0.0	41.2

8 surfaces, 4 plates.						20 surfaces, 10 plates.				
i.	A.	B.	Re- flec- ted.	Per cent. po- larization.		A.	B.	Re- flec- ted.	Per cent. po- larization.	
				Refl.	Refr.				Refl.	Refr.
0	28.3	28.3	28.3	0.0	0.0	49.4	49.4	49.4	0.0	0.0
5	28.6	28.0	28.3	0.9	0.3	49.7	49.1	49.4	0.6	0.6
10	29.2	27.4	28.3	3.4	1.2	50.4	48.3	49.3	2.1	2.1
15	30.2	26.3	28.3	7.4	2.7	51.5	46.9	49.2	4.7	4.6
20	31.9	24.5	28.2	13.1	5.1	53.3	44.9	49.1	8.5	8.3
25	33.9	22.4	28.2	20.3	7.9	55.8	42.2	49.0	13.9	13.4
30	36.3	19.8	28.1	29.2	11.4	58.7	38.2	48.4	21.2	19.7
35	39.5	16.5	28.0	41.1	16.0	62.2	33.0	47.6	30.9	27.9
40	43.7	12.1	27.9	56.4	21.8	65.9	26.3	46.2	42.4	36.7
45	48.3	7.4	27.9	73.3	28.1	69.8	17.1	43.4	55.9	46.6
50	53.5	3.2	28.3	88.7	35.1	74.1	8.5	41.3	79.5	56.4
55	59.4	0.5	30.0	98.3	42.0	78.4	1.0	39.7	97.5	64.5
57	62.1	0.0	31.0	100.0	45.0	80.4	0.0	40.2	100.0	67.2
60	66.3	1.7	31.0	95.0	48.9	82.8	2.4	43.6	94.5	71.0
65	72.5	7.6	40.0	81.0	54.1	86.9	18.6	52.7	64.7	72.2
70	79.0	25.0	52.0	51.9	56.3	90.4	45.5	68.0	33.0	70.0
75	85.3	48.2	66.7	27.8	55.8	93.5	69.9	81.7	11.3	64.6
80	91.0	71.1	81.0	12.3	52.6	96.2	85.9	91.0	5.6	57.1
82	93.1	79.0	86.0	8.2	50.5	97.1	90.3	93.7	3.6	54.0
84	95.0	85.5	90.2	5.2	48.6	97.9	93.7	95.8	2.2	50.9
85	95.5	88.4	92.0	3.8	47.5	98.3	95.1	96.7	1.6	49.3
86	96.8	91.3	94.0	2.6	46.2	98.7	96.4	97.5	1.2	47.7
87	97.6	93.8	95.7	1.7	45.0	99.0	97.4	98.2	0.8	46.1
88	98.4	96.1	97.2	1.1	43.8	99.4	98.4	98.9	0.5	44.5
89	99.2	98.1	98.6	0.5	42.5	99.7	99.2	99.4	0.2	42.9
90	100.0	100.0	100.0	0.0	41.2	100.0	100.0	100.0	0.0	41.2

These results are perhaps better shown in figs. 1 and 2, in which abscissæ represent values of i , and ordinates percentages of polarization. In fig. 1 the four highest curves represent the polarization of the beams reflected by 1, 2, 8, and 20 surfaces. The other four curves give the corresponding refracted beams. Fig. 2 gives all the curves of Table II. relating to 20 surfaces—the five curves corresponding to A, B, the intensity of the refracted beam, and the polarization of both the reflected and refracted beams. When $i=0$, both the reflected and refracted beams are unpolarized. With 10 plates of glass about half the light is reflected, the transmitted ray being but little brighter than that reflected. With 1 or 2 surfaces the reflected beam increases as i increases; with 8 surfaces it remains nearly constant up to 50° ; while with 20 surfaces a marked diminution is perceived. This very remarkable result may be expressed by saying that 10 plates of glass transmit more light obliquely than normally. The appearance to the eye confirms this result, but it deserves a careful photometric proof. At 57° the reflected ray is, of course, in all cases totally polarized; but at other angles the amount of polarization is greater the less the number of surfaces, instead of the contrary, as might have been anticipated.

With the refracted ray quite a different law holds. For 1 surface the polarization increases from 0° to 90° ; with 2 surfaces it becomes sensibly constant near 90° ; while with a larger number a distinct maximum is obtained. It is commonly supposed that the greatest effect is obtained at the angle of total polarization. But the maximum is sensibly beyond this, unless a very large number of plates are employed; and hence it seems probable that a bundle of plates, polarizing by refraction, would give the best results if set at a greater angle than 57° , as 65° or 70° . The transmitted ray, however, diminishes rapidly for large angles of incidence. A very large number of plates are required to render the polarization nearly complete, which accounts for the light always remaining when even the best polariscopes by refraction are crossed. At 90° all the refracted beams are polarized by the same amount of 41.2 per cent. Or, at grazing incidence, the amount of polarization is independent of the number of plates, one polarizing as completely as a hundred. This number, 41.2, may be obtained as follows. Differentiate the value of A in terms of i and r , and make $i=90^\circ$, when the refracted beam will equal $1-A=4\tan r\,di=3.376\,di$, since when $i=90^\circ$ $r=40^\circ 10' 7$. In the same way $1-B=\frac{8}{\sin 2r}\,di=8.115\,di$; and applying to them the formulæ for the polarization of the refracted beam, we find it equal to 41.2.

To show how far these effects are due to internal reflection,

another Table was computed by the formula $(1-a)^m$, or supposing that no internal reflection took place. A comparison showed that while the reflected beam is affected but little, a great change takes place in the transmitted light. The results are shown by the dotted lines in figs. 7, 8, and 10, and will be discussed below.

To test the above conclusions, two experimental methods may be employed. First, by means of a photometer, to determine the amount of light in any given case; and, secondly, by means of a polarimeter, to determine the percentage of polarization of the reflected and refracted rays. The latter method has been employed in the following experiments. The instrument commonly used to measure the amount of polarization was invented by Arago, and is called a polarimeter. It consists of a Nicol's prism and Savart's plates, in front of which are several glass plates, free to turn, and carrying an index which moves over a graduated circle, thus showing the angle through which they have been rotated. The prism and plates form a Savart's polariscope, which gives coloured bands with either light or dark centre, according as the plane of the prism is parallel or perpendicular to the plane of polarization. When the plates are so placed that the light passes through them normally, they have no effect on it; but when turned, they polarize it in a plane parallel to the axis of rotation, and by an amount dependent on the angle. Let the instrument be so set that the axis of rotation shall be perpendicular to the plane of polarization, and the plates set at zero. The bands will then be visible, the centre one being bright. As the plates are turned the bands become fainter, until the polarization neutralizes that originally present in the beam; beyond this point the bands reappear dark-centred. The amount of polarization is thus readily determined by turning the plates until the bands disappear, when the angle is reduced to percentages by means of a Table. The difficulty of computing this Table, however, is the real objection to the use of this instrument. It may be determined by the formulæ given in the first part of this paper; but it, of course, then fails to prove them. Moreover no account is taken of imperfect transparency, dust on the surface, and other sources of error. An excellent way of forming this Table experimentally is to view through the instrument a beam of light totally polarized. If now the plane of polarization of the beam is changed, the percentage of polarization will alter, being zero when it is inclined 45° to the axis of the plates, and wholly polarized at an angle of 0° or 90° . At any angle a the beam may be regarded as composed of two, $\cos^2 a$ polarized vertically, and $\sin^2 a$ polarized

horizontally. The percentage of polarization will therefore equal $\frac{\cos^2 a - \sin^2 a}{\cos^2 a + \sin^2 a} = \cos 2a$, from which the polarization corresponding to any given angle is readily determined.

The result of such a comparison is given in Table III. Four series of observations were taken, from which curves were constructed as in fig. 8, with angles of incidence as abscissæ, and percentages of polarization as ordinates. A curve was next drawn, coinciding with them as nearly as possible; and its ordinates are given in Table III. column 3; the angles of incidence are given in the first column, and the theoretical polarization in the second column of the same Table. Column 4 gives the differences; and from it we see that, while the agreement is very close between 0° and 60° , above this point a marked variation is perceptible. This deviation will be further discussed in connexion with fig. 8.

TABLE III.—Table for Arago's Polarimeter.

<i>i.</i>	Theoretical.	Empirical.	Difference.
0	0.0	0.0	0.0
20	5.1	5.0	-0.1
30	11.4	13.0	1.6
40	21.8	23.5	1.7
50	35.1	37.0	1.9
55	42.0	43.0	1.0
60	48.9	49.0	0.1
65	54.1	57.5	3.4
70	56.3	63.5	7.2
75	55.8	67.0	11.2
80	52.6	72.0	19.4

To avoid the defects of the above instrument, the following arrangement has been employed. A brass tube, A B (fig. 3), about a foot long, is closed at one end by a double-image prism, B, and at the other by a rectangular aperture, A, of such a width that its two images, as in the Arago polariscope, shall be in contact, but not overlapping. To the prism is attached a Nicol's prism, free to turn, and carrying an index, moving over a graduated circle, which shows how far it has been rotated. The tube is then mounted, so that it can be set at any altitude or azimuth, or rotated round its own axis; and three graduated circles serve to measure these quantities. In the instrument as actually constructed (fig. 4), the whole is supported on an upright, which terminates below in a large screw, C, by which it may be attached to a post or tree when used out of doors. A tube slips over this, which carries a cross piece forming a T;

and through the top of this passes the end of a second T, through the end of which the polarimeter slides. Three of these tubes are graduated to show the azimuth and altitude of the polarimeter tube, and the amount it is turned round its own axis.

The working of the instrument is as follows. If the Nicol's prism be removed and the light unpolarized, the two images of the aperture at the end will be equally brilliant. If now the Nicol's prism be replaced and turned, the images will vary in brightness, alternately disappearing at intervals of 90° . If the light is polarized, one image will in general be brighter than the other; but by turning the Nicol's prism, certain positions will always be found in which the two images will be equal. The percentage of polarization is then readily determined from the angle through which the prism has been turned. To determine the law which connects these two, let the plane of polarization be vertical, and the line of junction of the two images parallel to it. Then call A and B the brightness of the two images respectively, in which case the polarization $p = \frac{A-B}{A+B}$. If the prism be turned through an angle v , one image will have a brightness $A \sin^2 v$, the other $B \cos^2 v$; and if they are equal, $A \sin^2 v = B \cos^2 v$; hence

$$p = \frac{\cos^2 v - \sin^2 v}{\cos^2 v + \sin^2 v} = \cos 2v.$$

The amount of polarization is then very simply found by turning the Nicol until the images are equal, then reading the angle, doubling it, and taking the cosine. Evidently there are four positions of equality of the image; and in the following experiments all four were observed, reading to tenths of a degree, and the mean taken.

The results may be reduced by a Table of natural cosines, first multiplying the angle by 2. Evidently, when the light is unpolarized, the angle will be 45° ; when totally polarized, 0° . When the line of junction is inclined to the plane of polarization by an angle w , the observed polarization $p' = p \cos 2w$. This suggests a means of determining the direction of the plane of polarization. Make two observations of the amount of polarization, turning the polarimeter 45° . Then call p , p' , p'' the true and the observed polarization in the two cases, and w the unknown angle between the line of junction in its first position and the plane of polarization. Having given p' and p'' , we wish to determine p and w . Evidently $p' = p \cos 2w$, and $p'' = p \cos 2(45^\circ - w) = p \sin 2w$. Taking their quotient gives $\tan 2w = \frac{p''}{p'}$; and the sum of their squares gives $p = \sqrt{p'^2 + p''^2}$.

This method, though elegant theoretically, does not appear very accurate practically, as the plane is more accurately determined by covering the end of the polarimeter with a cap containing a plate of selenite, thus converting it into an Arago's polariscope. Then turn the tube until the two images have precisely the same colour, when their line of junction will be inclined 45° to the plane of polarization. The plane may also be determined, more easily but less accurately, by removing the Nicol's prism, and turning the tube until the two images are equally bright, and adding 45° to the reading.

We next wish to determine the delicacy of this instrument in different parts of its scale. If the Nicol's prism be set at an angle v' , differing slightly from its true value v , the brightness of the two images will be $A \sin^2 v'$ and $B \cos^2 v'$ respectively. Now it is commonly assumed that the difference in two such images will be perceptible when the difference in brightness, divided by the brightness of either, equals a certain fraction $\frac{1}{a}$, in which a equals about 80. By differentiation it may be proved that the error in p or dp corresponding to any value of a is given by the formula $dp = \frac{1-p^2}{2a}$, from which the error in the result for any unobserved difference in brightness of the two images is readily determined.

If $p=0$, $dp = \frac{1}{2a}$, its greatest value, which diminishes as the polarization increases, becoming zero when $p=0$. Hence the greater the polarization the more accurately it can be measured. If $a=80$, $dp = \frac{1}{160}$ for its greatest value; hence the instrument should always give results within two thirds of 1 per cent. Observation, however, shows that the error is much greater, a difference in brightness of $\frac{1}{10}$ being by no means perceptible.

The first series of observations were made on the light of the sky. The instrument was screwed into a post and levelled, the altitude and azimuth of the sun taken, and the instrument then directed towards the points to be observed. Most of these were in the same vertical plane with the sun, so that it was only necessary to determine their altitudes. The line of junction was then brought parallel to the plane of polarization—that is, turned until it was vertical, since it then lay in the plane passing through the sun. The four positions of the Nicol's prism, in which the two images were equally bright, were then observed, reading the angles to tenths of a degree, and taking the mean. The percentage of polarization was finally obtained as described above. Eleven series of observations were taken :—nine at Water-

ville, N. H., in a valley at a height of 1500 feet, surrounded by mountains 4000 feet high; the other two from the top of the Institute of Technology, Boston. It soon became evident that the polarization depended on the solar distance of the point under observation, and not on its altitude. This is well shown in fig. 5, in which abscissæ give the solar distances, and ordinates the polarization of the observed points. The sun's altitude varied in these series from 0° to 68° .

Before discussing these observations further, it seemed desirable to determine the polarization of other parts of the atmosphere not lying in the same vertical plane with the sun. Moreover, as the polarization of points at equal distances from the sun should be compared, the polarimeter was so mounted that its principal axis would pass through the sun. The two graduated circles would then give solar distances, and the angle from the vertical plane through the sun instead of altitudes and azimuths. The second of these angles will be called the meridian-distance, and will be regarded as positive to the right, and negative to the left of the sun. Of course, the direction of the axis should continually change, so as to follow the sun; but as great accuracy in the determination of the angles was not needed, it was found sufficient to readjust it every few minutes. Another advantage of this arrangement was, that the line of junction, being turned parallel to the axis, would always lie in the plane passing through the sun, and hence be parallel to the plane of polarization.

Five series of observations were made in this way, all tending to show that the polarization was independent of the distance of the point to the right or left of the plane passing through the sun. The results are given in Table IV., in which the first column gives the number of the series, the second the sun's altitude, the third its distance from the points observed, the fourth the number of the latter, and the fifth their mean polarization.

TABLE IV.—Polarization of Points equidistant from the Sun.

Series.	Altitude.	Distance.	No.	Polarization.
12	5°	90°	9	76.7
13	20	60	6	42.9
14	48	90	8	65.4
	45	60	10	41.7
	42	120	2	42.4
	40	30	8	10.7
15	40	90	10	60.4
	41	120	9	40.9
	43	60	10	34.2
16	-5	90	6	77.5

One series, the first, is given in full in Table V., to show the amount of variation of the different points observed.

TABLE V.

12. July 15. Sun's distance 90° , altitude 5° .

M. D.	Polar.
75°	78.6
60	74.3
45	79.8
30	77.0
0	77.2
30	74.8
45	75.2
60	76.8
75	76.8
Mean	76.7

All these observations point to one very remarkable result—namely, that the polarization is the same for a given solar distance for any meridian distance—in other words, that the polarization is the same for all points equally distant from the sun. The variations in the observations are to be ascribed partly to errors of observation and partly to real irregularities in the atmosphere, as it is evident that they follow no regular law. The means therefore give us the true polarization with much greater accuracy. They are represented in fig. 5 by small crosses. The next thing is to determine the law which connects the polarization with the solar distance in all these observations. A drawing was made like fig. 5 enlarged, and a fine copper wire laid on it and bent into such a shape that it should coincide as nearly as possible with all the observations. The ordinates for every 10° were then read off, giving the results entered in column 7 headed “Observed” of Table I.

A simple explanation of the polarization of the sky is to assume that it consists of molecules of air or aqueous vapour which reflect the light specularly, and whose index of refraction differs only by a very minute amount from that of the medium in which they float. The theoretical polarization would then be at once given by Table II., making i equal to one half the solar distance. The curve thus obtained is given in fig. 5 at A. The polarization, according to this, should be complete at 90° from the sun, while in reality it is only about 70 per cent. If, however, we multiply the ordinates of curve A by this fraction, we obtain curve B, which agrees almost precisely with the curve given in column 2 of Table XII. (*l. c.*). Its ordinates are given in column 3, and the differences in column 4. From the latter it

will be seen that the empirical curve gives results somewhat too great for solar distances less than 60° , and too small for greater distances; but the deviation is so small, compared with the accidental errors, that we are justified in regarding the agreement as complete within the limits of errors of observation.

It will be noticed that no account is here taken of the points of no polarization or neutral points of the sky; but the polarization is very slight for some distance from them, and hence is not easily measured. They must be regarded as due to some secondary disturbing cause, as refracted light, which alters the general polarization of the sky but little.

When the polarimeter is directed towards a polished coloured plane surface, the two images assume different tints. One, which contains the light polarized in the plane of incidence, or A, is composed mainly of the light reflected specularly, and is therefore white like the source of light. The image B contains but little of the light reflected specularly, consisting principally of the rays emitted by the body, and hence partaking of its colour. The idea at once suggested itself, that testing the light of the sky in this way might give a clue to the cause of its colour. The experiment was tried several times with negative results, the two images appearing of precisely the same blue tint. But on the evening of July 15th, near sunset, when measuring the polarization of a point near the northern horizon where the blue colour was comparatively pale, a marked difference in the two images was observable. The image A was found to be of a yellowish brown, B of a greyish blue or violet tint. This observation has since been frequently repeated, and can, in fact, be made almost any clear evening near sunset. Evidently we may conclude from these colours that the true colour of the sky-particles is blue, a view quite in accordance with the observations of Professor Cooke with the spectroscope, and of Professor Tyn-dall on aqueous vapour in a state of formation.

Observations were next made to test the results found above for the light reflected and transmitted by several parallel surfaces of glass. To check the results which are given in figs 6-10, two, and in some cases three independent methods were employed. To measure the polarization of the reflected ray, one or more sheets of glass were laid on a piece of black velvet and rendered horizontal with a spirit-level. The polarimeter was then mounted a short distance from them, carefully levelled, and then turned down so that the light should be reflected from their surfaces. Its angle of depression would then equal the complement of the angle of incidence. The line of junction of the two images was then rendered vertical, and the polarization measured in the usual way. The polarization of the sky, if

clear, would introduce a large error into the results; and care was therefore taken to make these observations only on cloudy days.

The second method was to replace the slit of a large Babinet's goniometer, or optical circle, by a Nicol's prism, which was free to turn round its axis, the angle of rotation being measured by a graduated circle and index. In the eyepiece of the observing-telescope a Nicol's prism was placed, and in front of it quartz wedges giving lines which were bright or dark-centred, according as the transmitted ray was polarized vertically or horizontally. On looking through the telescope, the field was seen to be traversed by lines, which disappeared only when the Nicol in the collimator was inclined 45° with the vertical. At any other angle, a , the vertical and horizontal components were $\cos^2 a$ and $\sin^2 a$, and hence were equivalent to a beam polarized vertically by an amount $\cos 2a$. If now any object was inserted between the two telescopes polarizing the light horizontally p , the bands would disappear only when $p = \cos 2a$. Measuring the four positions of disappearance and taking their mean, gave an accurate measure of the polarization by a Table of natural cosines, as with the polarimeter described above. Another way of expressing the effect of this instrument is to say that the bands disappear when the Nicol is so turned that the plane of polarization shall be brought by the object under examination to an angle of 45° . The method of measuring the polarization of the reflected ray is now obvious. The pieces of glass are placed vertically on the centre plate between the two telescopes, the latter set at an angle of $2i$, and the glass turned until the light is reflected from its surface, so as to render the field bright. The Nicol is then turned until the bands disappear, and its position recorded. The angle between the telescopes is then altered so as to make i successively 20° , 30° , 40° , &c., and the observation repeated. Various adjustments must be made to eliminate constant errors, but they need not be detailed here. One series was made with a glass prism having an index of 1.52, a second with a plate of coloured glass, a third with a sheet of plate glass, and others with four and ten microscope slides. The latter were used because the thickness of the plate glass was such that, when a number of plates were placed between the telescopes, a portion of the internal reflection would be lost.

To measure the polarization by refraction, two similar methods were employed. The plates were placed vertically over the centre of a graduated circle, and a piece of ground glass was viewed through them by the polarimeter. The plates were then set at various angles, and the polarization measured in each case. All these observations were made in cloudy weather, to eliminate the

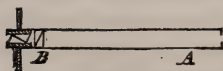
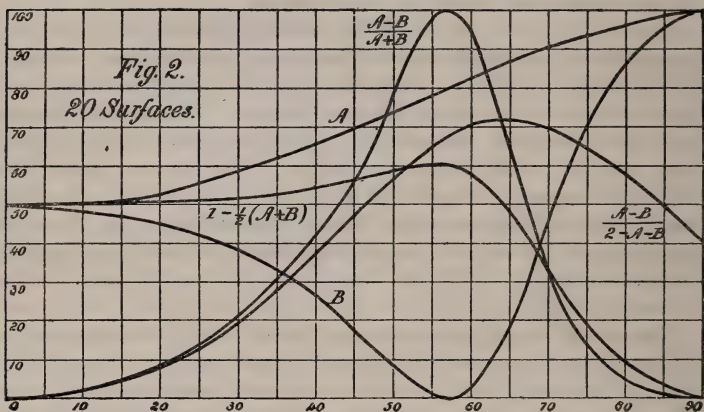
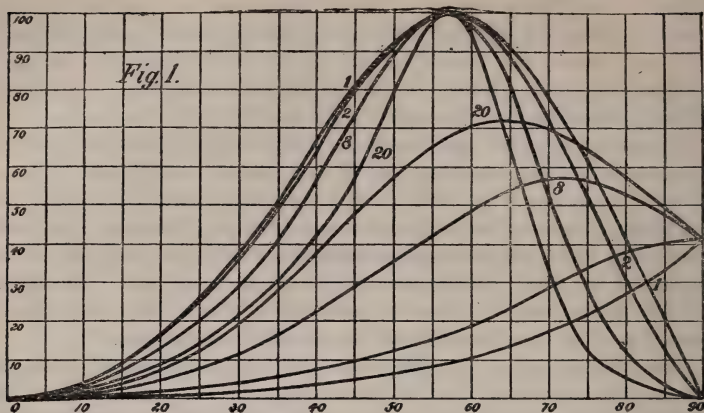
effect of sky polarization. Several lines were made with the optical circle and Babinet's wedges, placing the telescopes opposite each other, and recording the angles of the Nicol for various positions of the plates. Still a third method was employed, already described in connexion with the Arago's polarimeter.

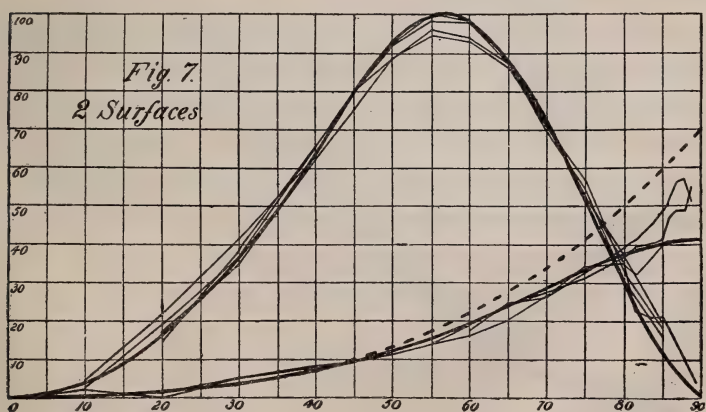
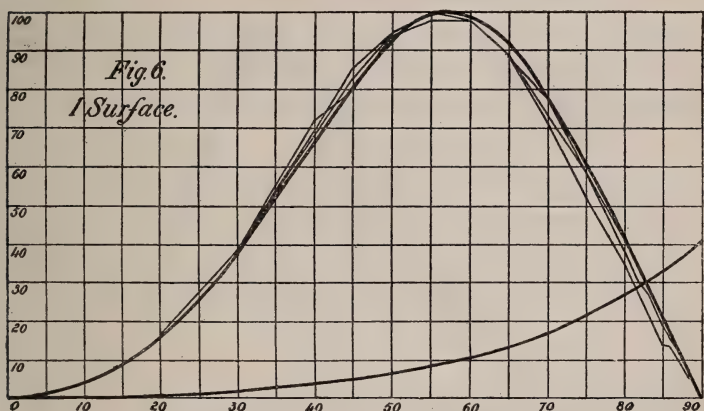
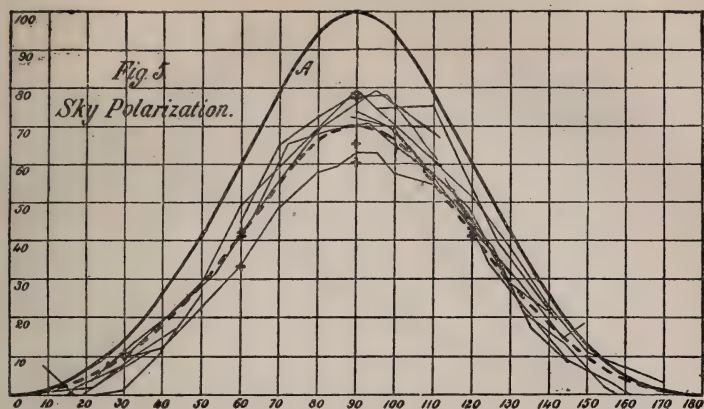
It will be noticed that no observations are given of the polarization of a beam transmitted by one surface of glass. There seemed to be no easy method of measuring this quantity. It might be done by making a series of prisms of such angles that when the light was incident on one face at 10° , 20° , 30° , &c., the refracted ray would strike normally on the second face. The effect of the latter would then be nothing; so that the polarization would in this case be entirely due to the first surface.

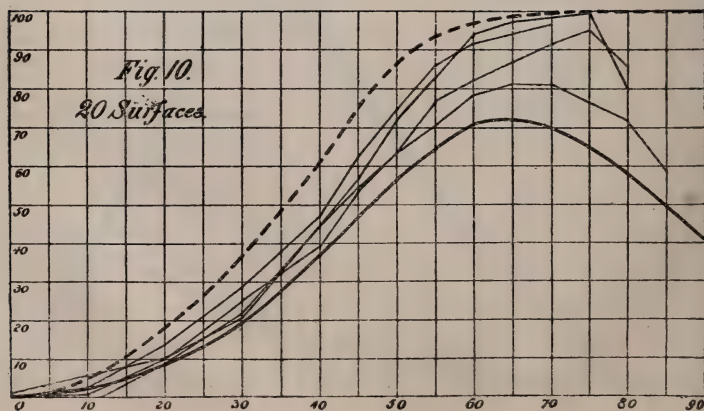
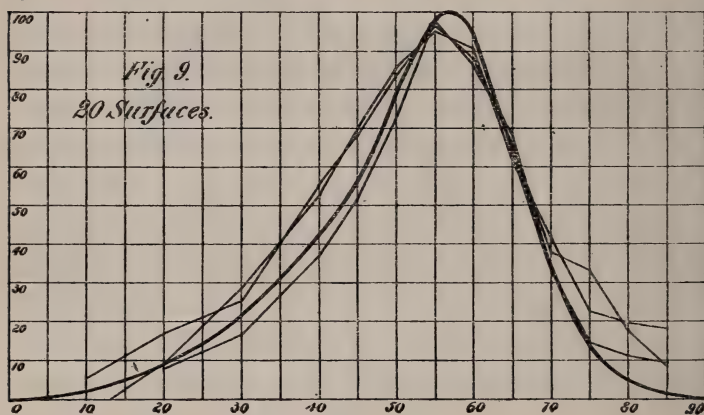
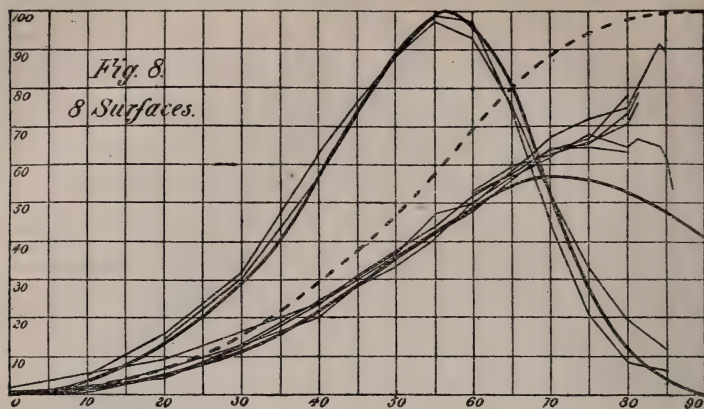
To determine the polarization of a single surface of glass, a plate was covered on one side with lampblack; but it was found to give the same results as when laid on velvet. A plate of coloured glass was therefore used instead.

Fig. 6 gives the results of the observations of the polarization effected by one surface of glass, fig. 7 that of two surfaces, fig. 8 of eight, fig. 9 the reflected beam for twenty surfaces, and fig. 10 the corresponding refracted beam. The smooth curves give the theoretical polarization, including the internal reflection, and the dotted lines omitting it. The observations of the reflected beam agree very well with theory, especially when the number of surfaces is small. The refracted beams, on the other hand, show a deviation from theory which becomes perceptible for one plate above 80° , for four plates above 65° , and for ten plates above 20° .

The errors most likely to occur, which would be common to all the observations on the refracted beams, are, first, stray light, or light entering the instrument without passing through the glass; secondly, light passing through the glass endwise, which might be recognized by its deep green colour; and thirdly, light reflected from the front surfaces of the plates. But all these errors would tend to diminish instead of increase the polarization; and hence, if eliminated, the divergence from theory would be still greater. Probably the true explanation is that internal reflection does not take place as completely as theory assumes, partly owing to the imperfect transparency of the medium, and partly to the dust and other impurities on the surface. It makes but little difference for the reflected rays, the polarization being the same for three values of i , namely, 0° , 57° , and 90° . For the refracted ray, on the other hand, the variations are very great, amounting in the case of twenty surfaces, at 90° incidence to over 50 per cent. From the dotted lines we see that a partial absence of the internal reflection would account

*Fig. 3.**Fig. 4.*





for all the results obtained, while neglecting it entirely would cause a still greater divergence between theory and observation.

On account of the thickness of the bundle of ten plates of glass, a portion of the secondary reflection would be thrown a considerable distance to one side, especially when i is large, so that it might fall quite outside of the instrument, or even be cut off by the ends of the plates. This effect would be least marked with the polarimeter, next with the Savart, and most of all with the optical circle, on account of the small aperture of the telescope. But this is just the order in which the observations stand, all of them falling between the two theoretical curves. These observations also show the effect to be expected from a bundle of plates when used to polarize light by refraction. If ten plates are employed, set, as is usual, at 57° , the polarization would be only 67.2 per cent. if internal reflection took place, but would be 95.2 if this were in any way excluded. We may in passing point out that an advantage might be expected in such a polariscope from an increase in the angle of incidence, the increased polarization probably more than making up for the loss of light and distortion induced by the increased obliquity of the incident rays.

The want of perfect transparency of the glass would also tend to increase the polarization by enfeebling the secondary reflection; and dirt or grease on the surface of the glass would produce the same effect. With eight or with two surfaces these disturbing causes are much less marked, except for large angles of incidence, and hence the agreement with theory much better.

XVII. *Remarks on the Analytical Principles of Hydrodynamics, in Reply to Professor Challis.* By ROBERT MOON, M.A., Honorary Fellow of Queen's College, Cambridge*.

WHILE appreciating the candour evinced by Professor Challis throughout our discussion, I cannot suffer to pass in silence his assertion† that my “hydrodynamical researches are founded on differential equations which are really the same as those employed by Mr. Earnshaw.”

Professor Challis's argument to prove this is founded upon a complete fallacy. For, taking part of my solution of the equation

$$\frac{d^2y}{dt^2} + \frac{1}{D} \frac{dp}{dx} = 0, \quad \dots \dots \dots (1)$$

* Communicated by the Author.

† See Phil. Mag. vol. xlvii. p. 27.

viz. the equation

$$p = -\frac{\alpha^2}{\rho} + \chi\left(v + \frac{\alpha}{\rho}\right), \quad . \quad . \quad . \quad (2)$$

he says:—"Putting on the left-hand side of this equation $f(\rho, v)$ for p , it will be seen, since $\chi\left(v + \frac{\alpha}{\rho}\right)$ is arbitrary in form and value, that v is an arbitrary function of ρ , and, by consequence, on substituting for v in $f(\rho, v)$, that p is also an arbitrary function of ρ ."

The step here taken is nothing other than eliminating p between (2) and the equation

$$p = f(\rho, v),$$

where $f(\rho, v)$ is absolutely identical with the right-hand side of (2); and no such result as $\text{funct.}(\rho, v) = 0$, or $v = \text{funct.} \rho$, can be derived from such elimination. For, when χ in equation (2) is spoken of as representing an arbitrary function, all that is meant is that in every case of motion the pressure may be expressed in terms of ρ and v in the manner indicated by (2), where χ represents some function or other depending upon the particular circumstances of the motion.

In any particular case of motion χ will have a definite value; and to assume that in this particular case *another* relation exists between p , ρ , v , viz. $p = f(\rho, v)$, where $f(\rho, v)$ is *not* identical with the right side of (2), is simply to beg the question at issue.

To put the matter in a different light—we have to start with, as Professor Challis admits,

$$p = f_1(x, t), \quad \rho = f_2(x, t), \quad v = f_3(x, t).$$

How can it be possible, in general, to eliminate *three* quantities between these three equations so as to obtain the result $v = \text{funct.} \rho$, or $p = \text{funct.} \rho$? It is true that under particular circumstances we may do this, and under those circumstances no doubt Mr. Earnshaw's equations and mine may coincide; but the ratio of the number of cases in which our equations differ to the number in which they agree must be simply infinite.

I observe that Professor Challis refers to my solution of (1) as *a* solution. I contend that it is *the* solution—the only one which the equation admits of.

I have no doubt that this could be shown directly, in the manner I have elsewhere adopted to show that Poisson's integral of the accurate equation of sound for motion in one direction is the most general solution of that equation*; but it may be proved much more compendiously as follows.

The solution consists of the three equations

* See Phil. Mag. for August last.

$$p = -\frac{\alpha^2}{\rho} + \phi\left(v + \frac{\alpha}{\rho}\right),$$

$$v + \frac{\alpha}{\rho} = \psi_1 \left\{ x - \frac{\phi'(\omega) - \alpha}{D} \cdot t \right\},$$

$$\frac{1}{\rho} + \int \frac{d\omega}{\phi'(\omega) - 2\alpha} = \psi_2 \left\{ x - \frac{\alpha}{D} \cdot t \right\},$$

in which the three functions ϕ , ψ_1 , ψ_2 are arbitrary. But in any particular case of motion, p , ρ , and v must follow some given law, *i. e.* must be expressible at a given time by definite functions of x ; from which circumstance, as I have elsewhere shown, the forms of ϕ , ψ_1 , ψ_2 can be determined; and, being so determined, the last three equations will represent the motion throughout the entire period of its continuance. And there can be no relations between p , ρ , v , x , t in the case of motion under consideration other than those above given; for if there were such, it would follow that definite values of the pressure, density, and velocity at a given time admit of those quantities having alternative, and therefore ambiguous, values during the subsequent motion; which is impossible.

6 New Square, Lincoln's Inn,
January 17, 1874.

XVIII. *On Galvanic Polarization in Liquids free from Gas.*
By DR. HELMHOLTZ*.

I TAKE leave to communicate to the Academy the results of a series of experiments which I have made upon the galvanic polarization of platinum. The experiments mostly required a very long time; and I therefore crave pardon if I am obliged to leave unanswered for the present a number of further questions which at the same time obtrude themselves.

It is known that when a Daniell's zinc-copper element is closed by a water-decomposition cell with platinum electrodes, a current arises of quickly diminishing force, which, in the usual way of arranging the experiment, becomes indeed after a short time very feeble, but does not entirely cease even after a very long time. We will call this the *polarizing* current. If we afterwards separate the decomposition cell from the Daniell's element, and connect its platinum plates with the voltameter, we obtain another (the *depolarizing*) current, which in the cell is opposite in direction to the polarizing current, and is likewise

* Translated from the *Monatsbericht der königlich preussischen Akademie der Wissenschaften*, 1873, pp. 587-597.

powerful at the commencement, under the ordinary conditions of observation, but mostly soon dwindles to imperceptibility.

It is this simple experiment to which my investigations refer. The question to be solved was, On what depends the apparently unlimited duration of the polarizing current? for in a series composed as above described, *unless other changes also occur in it*, the electrolytic conduction which results according to Faraday's law in the liquids cannot take place without infringing the law of the conservation of energy. That is to say, if no other equivalents of potential energy are expended, in such a series the mechanical equivalent of the heat generated in the circuit must be equal to the work equivalent of the chemical forces which become active and are expended in electrolysis. The latter, however, if the decomposition proceeds according to the law of electrolytic equivalents, is negative*, and therefore cannot be equal to a positive heat-work to be produced by the current. If, then, Faraday's law is exclusively valid, the decomposition of water, even in the minutest quantity, cannot be maintained continually by a Daniell's element. Indeed no disengagement of the gases of which water is composed is observed in the above-described experiment, however long the current lasts.

It is moreover to be remarked that not even by diffusion or any process similar to diffusion could the molecules of hydrogen and oxygen, which on the polarization of the plates are urged towards them, become free and, again unelectric, remove from them. Such an occurrence would finally give, as the result of the work, a water-decomposition for which no equivalent inciting force would be present in the Daniell's element. If, as is probable, on the polarization of the electrodes a changed arrangement of the atoms of hydrogen and oxygen enters, whether in the interior or at the boundary surfaces of the liquid, these particles are at all events kept in their places by chemical or electric forces of attraction till new forces come to aid of sufficient power to set them free. Whatever relations between electric and chemical attraction-forces may be assumed, if the law of the conservation of energy holds good, an electric attraction upon one of the elements, whose potential is sufficiently great to overcome the chemical affinity, can itself be again overpowered

* According to Andrews, 1 gramme of hydrogen in burning to water gives 33,808 heat units; according to Favre and Silbermann, 34,462. For each gramme of hydrogen, in the Daniell's element, 32.5 grm. of zinc are dissolved, and the equivalent amount of copper precipitated. This quantity of zinc, when it separates copper from combination with sulphuric acid, generates, according to Favre, only 23,205 units of heat. Corresponding to this, an electromotive force of at least $1\frac{1}{2}$ Daniell's element is requisite in order to maintain even the feeblest lasting decomposition of water.

only by a force of equal or greater equivalent of work, so as to make the attracted particle freely movable in the liquid.

But if the electromotive force of the Daniell's element produces in our case no visible decomposition of water, it yet produces polarization of the electrodes; and this is itself an equivalent of work; for the polarized plates are afterwards, when separated from the polarized galvanic element, able for a certain time to generate independently an electric current, therefore to develop heat in the conducting wire, or, with a suitable arrangement, to perform respectively all other forms of work which galvanic currents can perform. In the state of polarization we have evidently to do with a changed arrangement of the ponderable atoms and the electricities in the decomposition-cell and on its electrodes, on the particular nature of which we have no need here to make any more special assumptions or suppositions, as long as the question is only the consideration of the work-values. The state of polarization is to be regarded as a new state of equilibrium, to which the source of decomposition, under the influence of the electrizing of the electrodes, tends, and which, when the electricity accumulated in the electrodes can discharge itself, tends back again to the state of electrically neutral equilibrium. But since for the production of a changed equilibrium in a limited system of bodies (such as is the decomposition-cell) only a finite amount of work is ever necessary, the production of polarization must always give only a current of finite duration, or one whose intensity asymptotically approaches to zero, and the polarizing current could on the whole make only as much electricity flow in the one direction as the depolarizing in the opposite.

So far as this is the case (and my experiments show that in gas-free liquids and with gas-free electrodes such a condition can be very nearly attained) the decomposition-cell acts as a condenser of very great capacity. In fact, if (according to the usual mode of representation) negatively charged oxygen be supposed in the vicinity of one electrode, and positively charged hydrogen near the other, but so that an interchange of electricities between the electrodes and the said constituents of water is not possible, the corresponding quantity of the opposite electricity may accumulate on the electrode itself; and each electrode would then, with the liquid, form a condenser with vanishingly small thickness of the insulating layer and, just on this account, of enormous capacity. This analogy has recently been emphasized by Messrs. Varley* and Maxwell†.

* Proc. Roy. Soc. Jan. 12, 1871.

† A Treatise on Electricity and Magnetism (Oxford, 1873), vol. i. p. 322.

Indeed the phenomena which arise when a pair of polarizable plates are inserted in a circuit correspond in their main features with those which would be presented by a condenser of very great capacity. The polarizing current is the current which charges the condenser, the depolarizing that which discharges it. The capacity of the condenser has only to be imagined so great that its charging and discharge require perceptible periods of time (seconds or minutes). Mr. Varley has tried to measure the capacity of such a condenser; but the sequel will show that, unless extraordinary measures of precaution be used in the experiments, other occurrences may play an important part and influence in a high degree the final result.

The occurrences, then, in actual experiments with polarizable electrodes are distinguished from what happens in a well-insulated condenser by this—that the charging current has a much longer duration than the discharging one, diminishes more slowly, and never entirely ceases. In this respect a cell with polarizable platinum plates appears to resemble a condenser with an imperfectly insulating partition; and even the phenomena of residual electricity find their analogue in the reinforcement of polarization which occurs after each interruption of the current.

It would lie near to assume the same reason for the continuance of the charging current with a polarized decomposition-cell as for the imperfect insulation of a condenser, viz. the existence of a slight metallic conducting-power in the electrolyzable liquids; and this would include a limitation of the validity of Faraday's law. Before we draw such a conclusion, however, we must inquire whether other changes, which might have similar results, do not also occur in the liquid and the electrodes. And indeed it would be necessary here to think chiefly of the part which may be played by gases held in solution in the liquid, or, according to Graham's discovery, occluded in the metal of the electrodes.

It is well known that the galvanic polarization of a platinum plate which serves as the hydrogen electrode in a decomposition-cell is diminished or destroyed by direct contact with the oxygen of the air, by access of water containing air, and by the contact of liquids which contain chemically combined oxygen, but may give it up to the separating hydrogen.

The same holds for the oxygen polarization of a platinum plate when it is in contact with hydrogen dissolved in water or other chemical combinations which can take up oxygen.

Besides, we know, as discovered by Graham, that platinum is capable, though in a less degree than palladium, of taking hydrogen into its own mass. The reception of oxygen, which we know takes place in silver in a state of fusion, could not, it is

true, be directly proved chemically for platinum by Graham ; yet the phenomena of polarization to be described in the sequel seem to indicate that perfectly similar relations subsist for oxygen as for hydrogen, and that only the quantity of oxygen that can be occluded by platinum is much less than that of hydrogen.

If, then, an electric current traverses a water-decomposition cell whose liquid holds hydrogen in solution, or whose platinum electrodes have occluded it, at that electrode to which the current urges the oxygen this may again become water, a corresponding quantity of dissolved hydrogen from the water, or of occluded hydrogen from the electrode, being consumed for that purpose. On the other hand, instead of this hitherto free hydrogen (at least not chemically combined with oxygen), an equal amount of electrolytically separated hydrogen will reappear at the other electrode, and either be dissolved in the liquid or, if there be time and space for it, even be driven into the platinum electrode. Although herein electrolysis therefore takes place in the liquid, yet the two products of the electrolysis do not finally appear, but the last result is that free hydrogen disappears at or in one of the electrodes, and at or in the other appears in increased quantity. For this process, which plays a prominent part in polarization-currents, I would propose the name *electrolytic convection*. Hence, in this process, the electromotive force which drives the current has not also to perform the work against the forces of the chemical affinity of hydrogen and oxygen, which must be performed when water is to be finally separated into these its two elements ; and therefore electrolytic convection can be kept up by a feeble electromotive force which is quite inadequate actually to decompose water—for instance, the force of only a single Daniell element.

The same thing holds when the liquid contains oxygen, or if the platinum plates have occluded it. Then, through electrolytic convection, free oxygen disappears on one side, while the same amount is forthcoming on the other.

The hydrogen or oxygen in this way set free in the process of convection at one electrode, is, so far as it is not occluded in the electrode, evidently as free to diffuse itself in the liquid, to be carried away by currents of the same, and to be developed as gas when the liquid is saturated, as the gases which are developed by ordinary electrolysis. While it is being diffused in the liquid it may also arrive again at the other electrode to fall again under electrolytic convection, and in this manner may keep up in a continual circulation a certain intensity of electric current.

A Daniell element can thus, in a water-decomposition cell

with platinum electrodes, maintain an unceasing feeble current not merely when the liquid is in contact with air, but also in a perfectly closed vessel, if its electrodes are saturated with oxygen and its liquid holds oxygen in solution.

The apparatus with which I experimented in this direction was an hermetically closed voltameter connected with a mercurial air-pump, containing two large platinum plates of approximately 180 and 300 sq. centims. surface cylindrically bent towards each other, and having derivation outwards through platinum wires fused into them. The liquid in this voltameter extended downwards to the mercury of the pump, with which it was raised and lowered, while the gases which collected above the liquid could always be drawn off through a separate cock. It was thus possible at any time to restore above the liquid a vacuum containing only aqueous vapour, and gradually to clear the liquid of every trace of dissolved gas.

Saturation of the plates with oxygen is attained thus: during several days oxygen is developed on them by a feeble current conducted to them through a platinum wire as hydrogen-electrode. I have seen for weeks together a current maintained by electrolytic convection alone, under the influence of a limited stock of oxygen in hermetically isolated liquid. It is characteristic of the influence of the liquid herein, that every mechanical motion of it, but especially circulating motions which are called forth by changes of temperature, adds considerably to the intensity of the current. This as good as entirely fails in gas-free liquids.

Much more efficient than oxygen in this relation is hydrogen, because it can collect in very great quantity in the plates. When the plates and the liquid are copiously saturated with electrolytically developed hydrogen, such a decomposition-cell behaves towards feebler currents, for hours or even for days, as a non-polarizable element, similarly to a solution of silver between silver electrodes. In spite of its being inserted, measurements of resistance in its circuit can be effected with the most perfect accuracy; and it shows, after interruption of the battery-current, scarcely a trace of polarization. Hitherto I have succeeded better in producing this state of hydrogen saturation by employing dilute sulphuric acid as the electrolytic liquid than with distilled water.

The constancy of the current, however, comes to an end when, through convection of the hydrogen, its store in one of the plates begins to run short.

Under these conditions, by employing only one (but a well-conducting) Daniell element, development of hydrogen as a gas can be observed at the plate to which it is carried, and therefore

an apparent decomposition of water. That this happens has already been seen by previous observers, but without a very exact determination of the conditions.

The course of things is but little different when, without alteration of the state of the electrodes, the electrolytic liquid is exhausted of gas by keeping it for weeks in the vacuum of the mercury pump. I succeeded in making greatly diluted sulphuric acid so free from gas that, on pumping out, it would not free itself from the vessel, and would not break up under the negative pressure of a mercury column of 60 millims. height. But even when distilled water was employed, I have constantly been able to bring it to this—that the traces of air at any time still escaping from the water, in the course of three or four days no longer perceptibly augmented the pressure in the vacuum, the volume of which amounted to about one sixth of that of the liquid, and which contained only aqueous vapour.

Under these circumstances also, when the plates were copiously charged with one of the two gases, currents entered, which might last for several days, but finally sank to imperceptible intensity. The multiplier here employed showed a deflection of 1 degree when traversed by a current which in twenty-four hours was capable of developing only 0.03 cub. centim. of hydrogen. A second difference was, that, as already remarked, motion of the liquid failed to strengthen the current.

On the other hand, the influence of the gases occluded in the platinum plates showed itself very distinctly when I altered the quantity of the store of gas contained in both. To this end, at the original charging of the plates with oxygen I conveyed by electrolysis small quantities of hydrogen to the two plates in conducting connexion. The second electrode was the mercury alloyed with a little zinc; the electrolytic liquid was distilled water. The oftener this was done, the briefer became the duration both of the current produced in the voltameter by a Daniell element and of the depolarization-current after the removal of the element. The same course of current-intensities which at the commencement, with a copious charge of oxygen, was accomplished in twenty-four hours, was finally, with the plates cleared as much as possible, run through in eighteen minutes. But if, after this course was entered, more hydrogen was brought to the plates, the duration of the current again increased, because now the plates began to be charged with hydrogen. I believe, moreover, that herein the minimum of gas-charging of the plates has not yet been reached, because even with the minimum attained of duration of the current there remained a small difference of time in favour of the polarizing compared with the depolarizing current. It is, however, a very tedious labour to

produce this minimum, because gases move on with extraordinary slowness in metal when they are not impelled by any exterior electromotive force; to remove the last remains of them consumes on this account an extreme length of time.

To show this slowness of the gas-motion I adduce the following:—Polarized plates of platinum standing in the usual air-containing liquids apparently lose their polarization in a few hours, or even minutes, when in conducting connexion with each other. Indeed on this circumstance depended, for instance, the utility of the polarizable electrodes employed by E. du Bois-Reymond formerly for experiments on animal electricity. On the contrary, in a gas-free liquid polarization dies away at first, it is true, quickly, but later very slowly. I have in such a case observed in the multiplier the depolarizing current for sixteen days. From the electrolytic equivalents of the current used for the polarization of the plates and the depolarization-current afterwards still existing it followed that months must elapse ere so feeble a current as the last named was could remove the residue of the gas-charge still present.

The phenomena in polarization which are similar to those attending the residual electricity of a Leyden jar, are accounted for by the occlusion of the gases. When hydrogen is penetrating a platinum plate, if the current be interrupted a few seconds, the gas has time during this interruption to advance further into the interior, and thereby diminish its density in the superficial layers. If the circuit be closed again, the resistance to the entry of fresh hydrogen is thereby diminished, the current may become stronger. Conversely, the depolarization-current can remove the gas which has been driven to the surface; if we interrupt this current, the hydrogen slowly pressing up from within will be able to accumulate at the surface and reinforce its polarization. It is known that in a platinum plate, behind an oxygen polarization an older hydrogen polarization can exist simultaneously, which latter comes to light when the former has vanished, and *vice versâ*.

So far as I can see, the phenomena belonging here are without difficulty explained if we assume for the onward motion of the gases occluded in metals the same laws as for the conduction of heat.

Lastly, it is to be remarked that in these cases, after the current of the condenser has passed (that is, the electricity attached only to the surface of the plates is discharged), another current can only set in in proportion as gas presses from the interior of the metal to the surface. If this takes place only very slowly, the current-intensity in the circuit becomes as good as independent of the resistance of the latter; so that, in my experi-

ments, insertions of from 20 to 60 miles of telegraph-wire certainly made the needle of the multiplier go back for a few seconds, but then soon permitted it to return to its previous deflection. The resistance of the rest of the circuit amounted to only about 2 miles of the same wire; the behaviour of the permanent current-intensities which appeared in the circuit with alternating resistances was just as if there existed at the surface of the plates a passing resistance in comparison with which 40-60 miles of wire was vanishingly little. This enormous apparent transitory resistance, however, existed only for just the then present direction of the current; if a current of the opposite direction was called forth, no such resistance was present. This holds good not only for platinum electrodes polarized by a Daniell element nearly to the maximum, but also for such as have been again depolarized almost to the vanishing of the polarization, and have thus arrived as nearly as possible at their natural state.

XIX. Notices respecting New Books.

Treatise on Practical Solid or Descriptive Geometry: embracing Orthographic Projection and Perspective or Radial Projection. By W. TIMBRELL PIERCE, Architect, late Lecturer on Geometrical Drawing at King's College, London. With 85 Plates of Original Drawings. London: Longmans, Green, and Co. 1873.

THIS volume is a small quarto, consisting of the plates and a text of 86 pages. About two thirds of it are devoted to orthographic, and the remaining third to radial projection. The former part is strictly elementary, but it embraces all the points which under ordinary circumstances a student would wish to make out. After a brief view of the projection of points and lines, with a few examples, the author fairly begins the subject in the second chapter, on Straight Lines and Planes, which is in substance the same as the first section of the *Géométrie Descriptive* of Monge. It is followed by a good many applications—one chapter (the third) treating of the projections of the five regular solids, while the fourth shows how to determine the projections of a cube from various data, and to solve a few other problems. The remainder of this part of the work is mainly devoted to a number of questions of intersection and tangency of a plane with cylinder, cone, and sphere, and of the mutual intersection and tangency of the same solids with each other; though a few other cases are given, such as the section of an annulus by a plane, and the contact of a cone with a surface of revolution. The treatment is clear and accurate; and though the subject is not without its difficulties, the student who brings to it a moderate acquaintance with solid geometry will be able to read it without serious trouble.

The case is somewhat different in the second part, the subject

of which (radial projection) presents difficulties of a much higher order than those of orthographic projection; and Mr. Pierce's treatment by no means smooths down its asperities. Of course a reader who has worked conscientiously through the first part, has gone through a sort of preliminary discipline for the second; but even such a reader would find it, at the outset, very hard to master the first four pages and Plate I. to which they refer—a difficulty which is seriously increased by an unfortunate misprint at a critical point (line 18, p. 60), and another in the lettering of Plate I. It might perhaps be said that the author undertakes to treat the matter as a question of geometry, and therefore addresses himself to competent readers; and this, of course, justifies a severe treatment of the subject, but not unnecessary abstruseness. It is said that when Brook Taylor's treatise '*On the Principles of Linear Perspective*' was published, it was criticised by a most competent judge as being abstruse to all and unintelligible to artists, for whom it was specially written. Brook Taylor's book is easy reading compared with this part of Mr. Pierce's; still, it may be freely allowed that a student who masters the present work will know the principles of the subject, and their application to several important and to some very difficult questions.

The plates, which form just half of the volume, are well done, and are as a rule intelligible to any one who takes proper pains to make them out. They are called on the titlepage "*85 Plates of Original Drawings*;" which, we suppose, means that they are copies of drawings actually made by Mr. Pierce; of course many are substantially the same as what may be found in other works, *e.g.* in Mr. Bradley's '*Elements of Practical Geometry*,' a work to which our author refers in his preface.

That descriptive geometry is a subject of great importance in the constructive arts is much more commonly recognized than it was a few years ago; and works of merit exist on the subject in English, particularly Mr. Bradley's, mentioned above; but their number is not large, and the present is likely to be of great value to the student. It contains, in a convenient form and moderate compass, what all who take up the subject ought to know, and as much as most students would wish to learn*. We do not doubt that a second edition will be called for ere long; and in that case Mr. Pierce would greatly increase its value by suggesting exercises for the student to work out. To some extent this want has been anticipated by a very good but unpretending little volume by Messrs. Edgar and Pritchard, '*Note-book on Descriptive Geometry*.' However, it is much better for a student to have in a single text-book all he requires; and the want could be easily supplied. As it is he finds in the plates a complete solution to every question in the book, and is never thrown on his own resources. There are a few misprints in the book which will doubtless dis-

* Mr. Pierce proposes to show in a future work the application of the subject to the several arts of construction.

appear in the second edition. We have already mentioned two; a third occurs in the second paragraph of Art. 61, p. 39, where the word "plane" is dropped out. Moreover the substance of the paragraph is not strictly correct, even when the printer's error has been set right.

Catalogue of Stars observed at the United-States Naval Observatory during the years 1845 to 1871. Prepared for publication by Professor M. YARNALL, U.S.N. Washington, 1873.

This Catalogue, which forms Appendix III. of the Washington Observations for 1871, contains the places in right ascension and declination of 10,658 stars observed during a period of more than a quarter of a century with the transit instrument, meridian circle, prime vertical, mural circle, and equatorial of the observatory. The preliminary matter treats principally of the methods employed for the reduction, throughout the Catalogue, to the Besselian epoch 1860.0, when the sun's longitude was 280° . The Catalogue gives:—the designations or numbers of the stars observed as they stand in the catalogues named, with the magnitudes, the mean right ascension 1860.0 with the mean year of observation; the number of observations of each star with its annual precession in 1860; the mean declination, mean year of observation, number of observations, and annual precession in 1860.

The Catalogue contains a large number of small stars which were observed in consequence of Mr. James Ferguson, who had charge of the equatorial, requiring many for comparison with the objects observed by him. Mr. Ferguson having observed the stars of the Pleiades with the equatorial, and those in Præsepe having been observed with the same instrument by Professor Hall, many in both clusters were reobserved with the transit instrument and mural circle. The resulting right ascensions and declinations are inserted separately from those determined with the equatorial. We cordially recommend this Catalogue to the attention of amateurs, and the more so as so many telescopes of large aperture are now in active work in this country.

XX. *Intelligence and Miscellaneous Articles.*

ON A NEW DETERMINATION OF THE MECHANICAL EQUIVALENT
OF HEAT. BY H. SERRANO Y FATIGATI.

THE mechanical equivalent of heat had not, until now, been determined by the relation which subsists between the work expended to turn the disk of a Ramsden electrical machine and the electrostatic decompositions produced. The following are, in brief, the process I have employed to resolve the problem and the results which I have obtained.

To turn the disk and measure the work, I wound round the handle of the disk of a Ramsden machine two strings passing over two pulleys and carrying each a weight at its extremity—the one

of 17 kilogr., and the other of 22. These weights descended near two graduated rules. I have deducted from the calculated work (1) the work equivalent to the *vis viva* retained by the weights at the end of their fall, (2) the work consumed by the friction of the strings against the handle and in the pulleys.

On the other hand, I estimated as exactly as possible the heat developed by the cushions,—first by employing a good mercurial thermometer, and then by means of a thermoelectric pile compared with the preceding indications, and measuring approximately the specific heat of the cushions and the disk, taking into consideration the losses in the air. The quantity thus obtained was likewise deducted from the calculated work.

Finally, to measure the electrolyzation, I took two large test-tubes, graduated and furnished with platinum wires extending to their bottom, one of which was connected with the machine, while the other was in communication with the earth. These tubes were immersed in a large receiver containing acidulated water, and united by means of a long string wetted to serve as a conductor to the electricity without producing sparks.

The following are the mean results obtained in twenty-eight operations:—

<i>Result of twelve operations.</i>			
Hydrogen disengaged. gramme.	Calories of dissociation.	Work expended. kilogr.	Mechanical equivalent.
0·017	0·578	272·84	471·99
<i>Result of seven operations.</i>			
0·042	1·428	649·74	455·00
<i>Result of nine operations.</i>			
0·005	0·170	79·5	467·64
Mean equivalent			464·87

I should be glad to have these results controlled by other physicists.—*Bibliothèque Universelle, Archives des Sciences Phys. et Nat.* Nov. 1873, pp. 252, 253.

ON THE EXPANSIBILITY OF EBONITE BY HEAT.

BY F. KOHLRAUSCH.

Accidentally repeated observations that ebonite lids stuck fast in glass vessels, or even cracked them, led me to the idea that this material might have a considerable expansibility by heat. Expectation was exceeded by reality; for it was found to be about three times as expansible as zinc.

The coefficient for 1° was found to be

0·0000770 measured between 16°·7 and 25°·3,
0·0000842 „ „ 25°·3 „ 35°·4.

Thus the increase with the temperature is very considerable. The linear coefficient of expansion for the temperature t can be put

$$0.000061 + 0.00000076t.$$

Two strips of ebonite and sheet zinc, when rivetted together, curve very materially with even a moderate heating. A thin strip of ivory 20 centims. in length, cemented by means of isinglass to a similar one of ebonite, even without index, forms a delicate thermometer, seeing that its free end moves through several millimetres for one degree. The curvature in consequence of unequal expansion may be most simply demonstrated by the aid of a mere plate of ebonite; for, owing to its bad conductivity, it curves considerably when rapidly heated on one side.

The solid expansion of ebonite is, from the above numbers, at 0° equal to that of mercury; at higher temperatures it is still greater. It is possible that other kinds expand still more, so that as a curiosity a mercurial thermometer might be constructed whose scale sunk on being heated.

The great expansion may possibly be connected with the proportion of sulphur which ebonite contains: Kopp found* for the coefficient of sulphur 0.000061 at 30° . On the other hand, the contrast to soft caoutchouc is very remarkable.

I will mention one fact which was observed in the observation of expansion. The bar of ebonite, which was about a centimetre in thickness, after being heated required a considerable time before it assumed a constant length. Although the bad conductivity is doubtless the principal cause of this, I imagine that another phenomenon is also at work. Like the elastic change of form, the expansion by heat may also not take place instantaneously, but continue itself after the change of temperature, gradually becoming weaker. A few observations of Matthiessen's with glass rods (Poggendorff's *Annalen*, vol. cxxviii. p. 521) seem to point in this direction; probably this thermal afteraction, like the elastic, occurs in an eminent degree in organic substances.—Poggendorff's *Annalen*, No. 8, 1873.

ON THE DISCHARGE OF ELECTRIFIED CONDUCTORS.

BY J. MOUTIER.

Equilibrium of electricity at the surface of a system of conductors results, as Poisson has shown, from the following condition:—The resultant of the actions exerted by the different electric layers on any point in the interior of the conductors must be no action. If by m be designated one of the electric masses, affected by a sign according to its nature, by r its distance from a point situated within the conductor, the function

$$V = \sum \frac{m}{r}$$

* Pogg. *Ann.* vol. lxxvi. p. 156.

must then have a constant value for all points situated within one and the same conductor. The function V , introduced into analysis by Laplace, has been designated by Green under the name of the *potential function*.

If m and m' denote two masses of electricity of the same name situated at a distance r , each of the masses is repelled by a force equal to $\frac{mm'}{r^2}$; if the distance of the masses becomes $r+dr$, the sum of the elementary works of the repulsive forces is

$$\frac{mm'}{r^2} dr = -d\left(\frac{mm'}{r}\right).$$

The sum of the elementary works of all the repulsive forces is therefore equal to the increase of the function

$$W = -\sum \frac{mm'}{r},$$

in which the summation is extended to all the masses of electricity, each of them being supposed to be affected by a sign. The function W is designated, according to a notation borrowed from Gauss, by the name of the *potential of electricity*.

The works of Helmholtz and Clausius have particularly called attention to this function, which plays a considerable part in the phenomenon of the electric discharge; indeed the increase of the potential represents the work done in the electric discharge. M. Clausius has shown that the potential can be readily expressed by means of the charges and potential functions relative to each of the conductors. If V is the potential function on one conductor, and Q its charge,

$$W = \frac{1}{2} \sum VQ.$$

If V_0, Q_0 are the initial values, V_1, Q_1 the final values relative to one conductor,

$$W_1 - W_0 = \frac{1}{2} \sum V_0 Q_0 - \frac{1}{2} \sum V_1 Q_1$$

measures the work done in the partial discharge of the system of conductors; and when the conductors are restored to the neutral state, the work of the complete discharge is

$$\frac{1}{2} \sum V_0 Q_0.$$

The mechanical equivalent of the discharge is independent of the manner in which the discharge is effected; it depends only on the initial and final values of the potential; so that the sum of the effects of the electrical discharge remains the same, whatever may be the nature of the discharge.

M. Helmholtz has already estimated the *vis viva* gained by the electricity in passing from the surface of a conductor to an infinite distance; but he considered the potential of the electricity of a conductor a constant quantity, while in reality the potential on the conductor diminishes in proportion to the charge: this diminution has the effect of modifying the expression of the work produced in the discharge through air.

Let us consider an electrified conductor having a charge q , and suppose that a quantity of electricity dq escapes from the electrified body and disappears into the air. When this electricity dq passes from a level surface where the potential function has a value V , to the infinitely near level surface where the potential function has the value $V+dv$, designating by dn the infinitely small portion of normal comprised between the two surfaces at the point considered, the repulsive force exerted on dq is $-\frac{dV}{dn}dq$. The elementary work

of the repulsion, in passing from one level surface to the surface infinitely near is $-dVdq$. Consequently, when the quantity of electricity dq removes to infinity, the corresponding work has the value $V'dq$, if we call V' the potential function at the surface of the conductor, or, what is the same thing, in the interior of this conductor: this expression has already been given by M. Helmholtz.

But in proportion as the loss of electricity is effected, the charge on the conductor diminishes; it is the same with the potential function. The potential function V' is proportional to the charge q of the conductor; we can put $V'=aq$, a being a constant peculiar to the conductor. The work necessary to repel to infinity the quantity of electricity dq is $aq\,dq$. Consequently, calling the initial charge of the conductor q_0 , the work necessary for repelling all the electricity of the body to an infinite distance has for its expression

$$T=a\int_0^{q_0} q\,dq=\frac{1}{2}aq_0^2=\frac{1}{2}V_0q_0,$$

V_0 denoting the initial value of the potential function on the conductor.

Therefore the work consumed by the repulsion of all the electricity to infinity is equal to the potential of the electricity; or, in other terms, the mechanical equivalent of the external discharge is equal to the potential of the electricity. The result would evidently be the same for a system of electrified conductors.

The value of T is independent of the path pursued by the electricity which escapes from the conductors; it is easy to recognize that this value remains the same when two equal quantities of opposite electricities meet on their passage and recombine.

Let us in fact suppose that two quantities of electricity, $+m$ and $-m$, recombine in a point M to form neutral electricity; let V be the potential function in this point. Suppose that at the point M the electricity $+m$ is repelled by the force $-m\frac{dV}{dn}$; the portion of the work T necessary to remove $+m$ to infinity, starting from the point M , is Vm . The quantity $-m$, on the contrary, situated at the point M , is attracted; the attractive force has the same value as the repulsive force; and when $-m$ removes from M to infinity, the corresponding work is equal to the preceding and of the contrary sign. The value of T remains consequently the same, whether the two electricities recede to infinity or recombination takes place

at any point whatever. It is necessarily the same if recombination is produced upon a conductor.

This is the case presented, for instance, in the discharge from a Leyden jar. Let us consider a spherical jar: we will call r the radius of the sphere which is formed by the interior coating, e the thickness of the glass. If q denotes the charge of the interior coating at a given instant, dq the quantity of electricity repelled from the interior to the exterior coating when these two coatings are united by a conductor, the work effected in the repulsion of the quantity of electricity dq is

$$\int_r^{r+e} \frac{q \, dq}{r^2} dr = \left(\frac{1}{r} - \frac{1}{r+e} \right) q \, dq.$$

The work of the repulsion which corresponds to the quantity of electricity q_0 originally contained on the interior coating is

$$T = \left(\frac{1}{r} - \frac{1}{r+e} \right) \int_0^{q_0} q \, dq = \frac{1}{2} \left(\frac{q_0}{r} - \frac{q_0}{r+e} \right) q_0.$$

The factor contained in the parenthesis represents the potential function on the interior coating. We thus again find, in the particular case of the Leyden jar, the expression of the potential of the electricity.

M. Helmholtz was the first who applied the theory of the potential to the discharge of the Leyden jar. His researches have been completed by M. Clausius*; and the theory of the experiments of M. Riess can now be regarded as very satisfactory. It still remains, however, to inquire how the discharge is produced, independently of the value of its mechanical equivalent. M. Helmholtz, after explaining the heat disengaged in M. Riess's experiments, adds:—

“This law is easy to understand, provided the discharge of a battery be not represented as a simple movement of electricity in one direction, but as a series of oscillations between the two coatings, oscillations which become less and less continually until the *vis viva* is extinguished by the sum of the resistances”†.

We have just seen that the discharge may be represented by a movement of the electricity directed from one coating towards the other.

From the preceding may be deduced the demonstration of a theorem established by Gauss in the case of a single conductor, and generalized afterwards by M. Liouville for a system of conductors:—When conductors contain respectively equal quantities of the two fluids, all these conductors are in the neutral state. Indeed in this case the potential is *nil*; consequently the external discharge of the system of conductors cannot give rise to any work.—*Comptes Rendus de l'Académie des Sciences*, Nov. 24, 1873, pp. 1238–1241.

* *Théorie Mécanique de la Chaleur*, traduite par M. F. Folie, vol. ii. p. 45.

† *Mémoire sur la conservation de la force*, traduit par L. Pérard, p. 107.

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[FOURTH SERIES.]

MARCH 1874.

XXI. *On the Electric Resistance of Selenium.*
By the EARL OF ROSSE, D.C.L., F.R.S. &c.*

THE recently discovered fact of the diminution of the electric resistance of selenium in the crystalline state when exposed to the action of light or, possibly, of radiant heat, is one which naturally excites some interest beyond that arising from the curious and unexpected nature of the phenomenon considered by itself; for the possibility of selenium being applied to the measurement of light or radiant heat invests the discovery with a very general importance.

Mr. Willoughby Smith seems to have satisfied himself that light, not heat, is the active agent; but I have spoken of the latter as possibly the cause of the effect observed, as Lieutenant Sale's paper in the Proceedings of the Royal Society (although it is therein stated that selenium is affected by *light*, and again, that the change of resistance is not due to an alteration of temperature) might lead one to infer that the observed effect was due to radiant heat, not to light; for he says that the *actinic* rays produce no effect, but that it is at a maximum in the red rays, or beyond them, near the maximum of the heat-rays; and inasmuch as he appears not to have determined by means of the thermopile the relative calorific power of the various rays of his spectrum, nor even to have reduced his results to what they would have been if the normal or diffraction spectrum had been employed, the experiment is inconclusive as to the comparative

* Communicated by the Author.

sensibility of selenium to the various rays of the solar spectrum.

As it was the opinion of one or two friends of much experience in kindred questions in physical science that heat, not light, is the active agent, and that selenium might with advantage replace the thermopile in the measurement of radiant heat, I procured the necessary apparatus for a preliminary trial.

Although my experiments are hardly sufficient to justify a decided opinion in favour of or against its applicability to photometric purposes, I think that they remove all hope of selenium replacing the thermopile in the measurement of heat of low refrangibility.

I exposed a rod of selenium during a sufficient interval to obtain the maximum effect from the light of a candle $3\frac{1}{2}$ inches distant. The diminution of resistance produced amounted to 24.3 per cent. A second trial gave a diminution of 24.2 per cent., the candle being at 4 inches distance. A vessel of hot water of about 9 or 10 inches diameter and 9 inches height produced no perceptible change when it stood for several minutes with its centre at 13 inches distance, nor even when it was brought 6 inches nearer. On the other hand, by means of the thermopile a deviation to the extent of 136 divisions of the scale of a Thomson's galvanometer was produced by the radiation from the vessel of hot water whose centre was at 13 inches distance; and when it was replaced by the candle at 4 inches distance, the deviation was increased to only 315 divisions. Thus it was shown that the two instruments were not comparable as measurers of the radiation from the two sources of heat.

The change of resistance produced by exposure to the radiation from a candle when a sheet of glass was alternately interposed and removed was next measured. The effect due to absorption by the glass appeared to be small, certainly not much more, possibly even less than the average absorption of light by glass. Perhaps 90 per cent. was transmitted. Owing to the inconstancy of the light, much accuracy was not readily obtainable in this experiment. The piece of glass had been shown by the thermopile to transmit 80 per cent. of the solar rays, and under 1 per cent. of the radiant heat from a blackened tin vessel of hot water. It was now by the same means ascertained that 48 per cent. of the radiant heat from a candle was transmitted by it.

A glass cell filled with a solution of alum was now taken; and it was found that, while the exposure of the selenium bar to the radiation from a naked candle at $3\frac{1}{4}$ inches distance produced a diminution of resistance amounting to 9.4 per cent., when the alum solution was interposed the decrease was still 8.95 per

cent.; hence the quantity transmitted by the glass cell and the alum solution it contained was as much as 95·2 per cent., as measured by the rod of selenium.

The same, measured by the thermopile, was found to be 7·06 per cent.

Having satisfied myself as to the comparative, if not the absolute insensibility of a bar of selenium to radiant heat of low refrangibility, and therefore as to its being unsuitable for replacing the thermopile in the measurement of those rays, I made a few experiments with the view of finding whether it was likely to be a suitable instrument in photometry.

To obtain a more constant light I substituted a paraffin lamp for the candle; and by placing in front of the horizontal selenium bar a vertical slit whose breadth could be varied at pleasure, and which could be closed before and during every alternate measurement of the resistance, also by varying the distance of the lamp, it was found that while the decrease of resistance varied as the breadth of the slit, and therefore as the length of the portion of the bar exposed to light, it was far more nearly proportional to the reciprocal of the distance, and therefore to the square root of the intensity of the incident light than to the intensity simply, between the limits within which the observations were made. The decrease of resistance observed was:—

Distance from lamp to centre of bar.	Decrease.
inches.	per cent.
2½	38
5	27
7	22
9	21
13½	15
19	11
22½	10
44½	3·77
75	1·57

The length of the bar was 2¼ inches.

No experiments were made to ascertain how far the sensibility was affected by the temperature of the room; but the presence of moisture rendered the action feeble and uncertain as the dew-point was approached, probably owing to the deposit of a very slight film of moisture on the surface of the bar. Another effect, probably to be ascribed to the same cause, was noticed on two occasions—a diminution of the change of resistance after a certain duration of exposure to a screening from the light.

		[terminations.]	
Ratio of resistances	1 min.	after interpo-	·663 : 1·0, mean of two de-
	2 "	sing and the	·802 : 1·0 "
	3 "	removal of	·817 : 1·0 "
	4 "	the screen,	·854 : 1·0, mean of three.
	5 "		·868 : 1·0 "

In a drier atmosphere the maximum, which was still as rapidly attained, was retained with tolerable constancy. Probably the diminution of the effect after the maximum had been reached may have been due to the slower dissipation of the film of moisture, and to its re-formation under the alternations of temperature which accompanied the alternations of light and darkness.

I regret that I shall not have leisure to pursue this subject for some time, and am therefore obliged to leave off these experiments in their present incomplete state.

January 1874.

XXII. *Note on the Composition of certain Mine Waters.* By J. ARTHUR PHILLIPS, *M.Inst.C.E., F.G.S., F.C.S., &c.**

SINCE the publication of a paper in which I attempted to show that the waters of the thermal spring at Huel Seton Mine are probably derived from the sea†, two other waters from deep Cornish mines have been analyzed in my laboratory.

The first of these was from the 212-fathom level at the Phoenix Mines near Liskeard, where it issues from the lode at a temperature of 65° F. This mine is in granite, and at one time produced large quantities of copper ores, but has for the last ten years been principally worked for tin, which is obtained from the same veins which were formerly wrought for copper. A large portion of the vein above the point from which the water for analysis was collected had been removed some years previously.

The following results, in grammes per litre and grains per gallon, were obtained by analysis.

* Communicated by the Author.

† Philosophical Magazine, July 1873

Water from the Phoenix Mines.

Solid matter ·2130 gramme per litre, or 14·91 grains per gallon. Sp. gr. = 1·0002.

Analytical Results.

	Gramme per litre.		Grains per gallon.	
	I.	II.	I.	II.
Carbonic acid.....	·0706	·0710	4·94	4·97
Sulphuric acid	·0438	·0430	3·07	3·01
Silica	·0304	·0310	2·13	2·17
Chlorine.....	·0234	·0226	1·64	1·58
Copper	·0008	·0008	·06	·06
Manganous oxide	·0005	·0005	·03	·03
Ferrous oxide.....	·0042	·0046	·29	·32
Lime	·0142	·0146	·99	1·02
Alkaline chlorides	·1642	·1618	11·49	11·33
Potassa	·0129	·0125	·90	·87
Soda	·0549	·0544	3·84	3·81
Ammonia	·0001	·0001	·01	·01
Nitric acid	·0016	·0016	·11	·11

The foregoing results may be thus tabulated:—

	Gramme per litre.		Grains per gallon.	
	I.	II.	I.	II.
Calcium carbonate.....	·0232	·0224	1·62	1·57
Ferrous carbonate	·0068	·0074	·48	·52
Manganous carbonate	·0008	·0008	·06	·06
Calcium sulphate	·0029	·0051	·20	·36
Sodium sulphate	·0748	·0710	5·24	4·97
Sodium chloride	·0367	·0356	2·57	2·49
Cupric chloride	·0017	·0017	·12	·12
Ammonium chloride	·0003	·0003	·02	·02
Potassium silicate (K^2SiO^3) ..	·0256	·0246	1·79	1·72
Sodium silicate (Na^2SiO^3) ..	·0415	·0447	2·90	3·13
Sodium nitrate... ..	·0025	·0025	·17	·17
Total by addition	·2168	·2161	15·17	15·13
Total found directly	·2130		14·91	
Excess of carbonic acid ...	·0575	·0580	4·03	4·07

The second water examined was collected at the 302-fathom level at Dolcoath near Camborne; this mine also formerly produced large quantities of copper ores. These have in depth gradually given place to cassiterite, and Dolcoath is at the present time the most productive tin mine in the United Kingdom. The water analyzed was collected from the roof of a short crosscut, in granite, 25 fathoms east of the engine shaft and 15 feet south of the main lode. The water issued in considerable quantities,

at a temperature of 92° F.; and a large portion of the upper parts of the vein had been removed by stoping.

The following results, in gramme per litre and grains per gallon, were obtained by analysis:—

Water from Dolcoath.

Solid matter ·6710 gramme per litre, or 46·97 grains per gallon.

Sp. gr. = 1·0007.

Analytical Results.

	Gramme per litre.		Grains per gallon.	
	I.	II.	I.	II.
Carbonic acid.....	·0588	·0578	4·12	4·05
Sulphuric acid	·1304	·1294	9·13	9·06
Silica	·0295	·0302	2·06	2·11
Chlorine.....	·1906	·1906	13·34	13·34
Arsenic	trace	trace	trace	trace
Alumina	·0004	·0004	·03	·03
Ferric oxide	·0044	·0044	·31	·31
Manganous oxide	·0006	·0006	·04	·04
Copper	·0007	·0007	·05	·05
Lime	·1040	·1028	7·28	7·20
Magnesia	trace	trace	trace	trace
Alkaline chlorides	·4192	·4212	29·34	29·48
Potassium	·0275	·0276	1·92	1·93
Sodium	·1442	·1449	10·09	10·14
Lithium	trace	trace	trace	trace
Ammonia	·0003	·0003	·02	·02
Nitric acid	·0017	·0017	·12	·12

The foregoing results may be thus tabulated:—

	Gramme per litre.		Grains per gallon.	
	I.	II.	I.	II.
Calcium carbonate.....	·0554	·0550	3·88	3·85
Ferrous carbonate	·0064	·0064	·45	·45
Manganous carbonate	·0010	·0010	·07	·07
Ferric arsenate	trace	trace	trace	trace
Calcium sulphate	·1773	·1749	12·41	12·24
Magnesium sulphate.....	trace	trace	trace	trace
Aluminium sulphate (3SO ⁴ Al ²)	·0013	·0013	·09	0·9
Sodium sulphate	·0448	·0454	3·14	3·18
Sodium chloride	·3118	·3118	21·83	21·83
Cupric chloride	·0015	·0015	·10	·10
Lithium chloride	trace	trace	trace	trace
Potassium silicate (K ² SiO ³).....	·0543	·0545	3·80	3·81
Sodium silicate (Na ² SiO ³)	·0171	·0183	1·20	1·28
Sodium nitrate	·0027	·0027	·19	·19
Ammonium chloride	·0009	·0009	·06	·06
Total by addition	·6745	·6737	47·22	47·15
Total found directly	·6710		46·97	
Excess of carbonic acid.....	·0316	·0308	2·21	2·16

The water issuing from the back of the crosscut at Dolcoath deposits in considerable quantities a soft reddish-grey precipitate which frequently assumes stalactitic forms. Similar incrustations, although generally less abundant, are found in nearly all deep mines wherever water issues from the vicinity of a vein and flows over the surface of the adjacent rock.

Three analyses made of air-dried specimens of this substance afforded the following results :—

	I.	II.	III.
Ferric oxide	36·30	36·29	37·75
Manganic oxide	trace	trace	trace
Arsenic acid	32·47	32·52	32·55
Arsenious acid	trace	trace	·68
Sulphuric acid	2·65	2·51	2·52
Water { combined	12·77	12·52	11·45
{ hygroscopic	15·90	15·98	15·20
	100·09	99·82	100·15

Nos. I. and II. are duplicate analyses of the same specimen, made in my laboratory ; No. III. is an analysis of another specimen, made, at my request, by my friend Mr. Dugald Campbell.

It may appear somewhat remarkable that a plentiful deposit of arsenate of iron should be formed from water in which only traces of arsenic could be detected, and in which the amount of iron present is so inconsiderable. On examining, however, the jars in which the water had been collected for analysis, a small amount of a flocculent brownish deposit was found at the bottom of each ; this, on being thrown on a filter and subsequently analyzed, was found to contain 31·52 per cent. of arsenic anhydride and 25·27 per cent. of ferric oxide. The ferric arsenate, together with a little clay, had been deposited before the clear water was siphoned off for analysis ; this accounts for the small amounts of iron and arsenic retained in solution.

These waters differ materially from those of the lithium spring at Huel Seton, which contain above a thousand grains of solid matter per gallon, while those from the Phoenix Mines and from Dolcoath contain respectively but 14·91 and 46·97 grains per gallon. The Huel Seton water is believed to be derived from the sea by percolation through a fault ; the waters, of which analyses are now given, are probably the result of the infiltration of surface-water through the workings of the mines, and through fissures in the respective veins.

XXIII. *On Ocean-currents.*—Part III. *On the Physical Cause of Ocean-currents.* By JAMES CROLL, of the Geological Survey of Scotland.

[With a Plate.]

[Concluded from p. 122.]

The Gibraltar Current.

IN my last paper I proved that it was only the water lying above the level of the submarine ridge which crosses the Strait of Gibraltar that could exercise any influence in producing circulation between the Atlantic and the Mediterranean*. The water of the Atlantic below the level of this ridge might be as light as air, and that of the Mediterranean as heavy as molten lead; but this could produce no disturbance of equilibrium. It is only the waters lying above the level of this ridge in the two seas that require to balance each other; and if there is no difference of density between the Atlantic and the Mediterranean waters from the surface down to the level of the top of the ridge, then there is nothing that can produce the circulation which Dr. Carpenter supposes. This submarine ridge comes up to within very nearly 100 fathoms of the surface; and according to Dr. Carpenter's own admission, little or no difference of density exists down to that depth†; it follows therefore that there is nothing to produce disturbance of equilibrium, or any such circulation as that which he infers. It is true that in his last expedition he found the bottom-water on the ridge somewhat denser than Atlantic water at the same depth, the former being 1·0292 and the latter 1·0265; but it also proved to be denser than Mediterranean water at the same depth. He found, for example, that "the dense Mediterranean water lies about 100 fathoms nearer the surface over a 300-fathoms bottom, than it does where the bottom sinks to more than 500 fathoms" (§ 51). But any excess of density which might exist at the ridge could have no tendency whatever to make the Mediterranean column preponderate over the Atlantic column, any more than could a weight placed over the fulcrum of a balance have a tendency to make the one scale weigh down the other.

Although Dr. Carpenter has done me the honour to discuss nearly all the objections which I have advanced against his theory, he nevertheless makes no reference whatever to this objection; and this is the more singular, seeing that the expedition, of which his memoir is a report, was chiefly if not solely undertaken for the purpose of establishing the correctness of his theory of the Gibraltar current. If, therefore, there was any one objection advanced by me which he might have been expected to

* Phil. Mag., October 1871, pp. 269-272.

† Proc. Roy. Geog. Soc. Jan. 9, 1871, § 13.

discuss, it was surely that relating to the Gibraltar current; for if the objection referred to be sound, it shows the mechanical impossibility of his theory. It proves that whether there be an under current or not, or whether the dense water lying in the deep trough of the Mediterranean be carried over the submarine ridge into the Atlantic or not, the explanation offered by Dr. Carpenter is one which cannot be admitted. It is incumbent on him to explain either (1) how the almost infinitesimal difference of density which exists between the Atlantic and Mediterranean columns down to the level of the ridge can produce the upper and under currents carrying the deep and dense water of the Mediterranean over the ridge, or (2) how all this can be done by means of the difference of density which exists below the level of the ridge.

We shall now pass to the consideration of Dr. Carpenter's objections to my conclusions regarding the influence of the Gulf-stream on climate.

Dr. Carpenter's Objections to my estimate of the Thermal power of the Gulf-stream.

"The doctrine of the extension of the Gulf-stream proper to the polar area, carrying with it a vast amount of equatorial heat, has been advocated with great ability by Mr. James Croll; who, employing the modern method of computing *units of heat*, essays to prove that the quantity of heat carried from the equatorial area by the Gulf-stream is so enormous, as to be competent not only to do all that Dr. Petermann attributes to it, but a great deal more" (§ 99).

"Without attempting to follow Mr. Croll through his calculations, I may stop to point out what appear to me to be the fallacies of his method; since if this can be proved erroneous, Mr. Croll's great array of figures is utterly valueless" (§ 100).

Now, in order to show that my "array of figures is utterly valueless," it is necessary to prove *either that I have overestimated the amount of heat conveyed by the Gulf-stream, or the effects produced by that heat.* This is evident; for if I am correct both as to the amount of heat conveyed and the effects produced by that heat, the figures must possess all the value which I claim for them.

We shall now consider how Dr. Carpenter manages to render my estimate of the heat conveyed by the stream "utterly valueless."

"In the *first* place, in Mr. Croll's preliminary comparison of the temperatures of the northern and southern hemispheres, he altogether ignores the influence on the distribution of heat over the globe which is exerted by the great relative preponde-

rance of land in the northern hemisphere And to affirm, as Mr. Croll does, that the lower mean temperature of the southern hemisphere is due to the amount of heat transferred over from that hemisphere to the northern by ocean-currents, is to repudiate all that has been established by the researches of meteorologists, as to the relative effects of land and sea," &c. (§ 101).

But what has all this to do with my estimate of the quantity of heat conveyed by the Gulf-stream? Supposing it to be true that I "altogether ignore the influence on the distribution of heat over the globe which is exerted by the great relative preponderance of land in the northern hemisphere," and supposing it to be true that the lower mean temperature of the southern hemisphere is not due, as I have concluded, to the amount of heat transferred over from that hemisphere to the northern by ocean-currents, this cannot in any way affect the value of my figures regarding the amount of heat conveyed by the Gulf-stream. It is not true, however, that I ignore and repudiate all that has been established as to the effect of land and sea in producing the difference of mean temperature between the two hemispheres. So far from this being the case, I have devoted an entire paper (Phil. Mag. Sept. 1869) to an examination of the arguments which have been advanced to explain the lower mean temperature of the southern hemisphere. I have given my *reasons* for concluding that an enormous amount of heat is transferred from the southern hemisphere to the northern by means of currents. These reasons may or may not be satisfactory; but nevertheless they are *reasons*, not *assumptions*. It would be needless as well as out of place to repeat these arguments; but I may be permitted simply to refer to one of them, viz. my reason for concluding that a great portion of the heat possessed by the Gulf-stream is derived from the southern hemisphere. If all that heat came from the northern hemisphere, it could only come from that portion of the Atlantic, Caribbean Sea, and Gulf of Mexico which lies to the north of the equator. The entire area of these seas, extending to the tropic of Cancer, is about 7,700,000 square miles. Were the heat which passes through the Straits of Florida derived exclusively from this area, the following Table would then represent the relative quantity per unit surface possessed by the Atlantic in the three zones, assuming that one half of the heat of the Gulf-stream passes into the arctic regions, and the other half remains to warm the temperate regions* :—

From the Equator to the Tropic of Cancer . .	773
From the Tropic of Cancer to the Arctic Circle . .	848
From the Arctic Circle to the North Pole . .	610

* See Phil. Mag. for October 1870, p. 258.

If a very large proportion of the heat possessed by the Gulf-stream be not derived from the southern hemisphere, these figures show that the Atlantic, from the equator to the tropic of Cancer, should be as cold as from the tropic of Cancer to the North Pole. But independently of this, a mere glance at a chart of ocean-currents will show that the Gulf-stream is chiefly fed by a current from the southern hemisphere. Without such a transference of heat it would be impossible to account for the N. Atlantic being actually 5 degrees warmer than the S. Atlantic.

Again, Dr. Carpenter remarks:—"In computing the heat imparted by the sun to the equatorial area from which the Gulf-stream is fed, Mr. Croll assumes that the heat, being wholly taken up by the water of the ocean, is transferred by its currents towards the polar regions; whilst of the heat which falls upon the land, a very large proportion is lost by radiation, passing off into the stellar spaces" (§ 102).

But this cannot in any way affect the correctness of the result of my computation of the amount of heat conveyed by the Gulf-stream. What I have maintained in my papers is, that the quantity of heat conveyed by the winds from intertropical land is trifling to that which is conveyed by currents from intertropical seas. Dr. Carpenter says that "the heat lost by *evaporation* from the sea must be far greater than that lost by radiation from the land." According to the laws of radiation and absorption, all the heat received from the sun by the land must be either reflected or radiated from its surface, with the exception of the small portion which is communicated to the air in *contact* with that surface. In fact it is by radiation that the sea as well as the land loses the greater part of its heat—the only difference in the two cases being, that heat radiated from the sea is absorbed more readily by the air than heat radiated from the land, and consequently produces a greater influence on climate.

Dr. Carpenter continues:—"Mr. Croll leaves almost entirely out of the question the N.E. transportation of an enormous amount of heat from the general surface of the Atlantic by the agency of the aqueous vapour thus raised; although the importance of this agency has been insisted on by the most eminent authorities in meteorology" (§ 103).

Here again, however, my estimate of the heat conveyed by the Gulf-stream, or the effects which it produces, cannot possibly be affected by the above consideration. It seems to be forgotten in this objection, that, were it not for the Gulf-stream, the quantity of heat which could possibly be derived from the Atlantic would be so much the less by an amount equal to that conveyed by the stream. Besides all this, there may be other

sources of heat than those noticed by me; but the omission does not diminish the importance of those to which I have referred. Had I been writing a treatise on meteorology, I should no doubt have referred as well to the influence of aqueous vapour as to many other sources of heat which I have purposely omitted in my paper on Ocean-currents as being foreign to my inquiry.

Dr. Carpenter objects to my statement that "the greater part of the moisture received at the equator is condensed and falls as rain in those regions," and refers me to the case of the Red Sea, where, although evaporation is excessive, almost no rain falls. But is it not an established fact, that the greater part of the water evaporated in intertropical regions does actually fall as rain in those regions? The reason why the vapour raised from the Red Sea does not fall in that region as rain, is no doubt owing to the fact that this sea is only a narrow strip of water in a dry and parched land, the air overhead being too greedy of moisture to admit of the vapour being deposited as rain. But over a wide expanse of ocean, where the air above is kept to a great extent in a constant state of saturation, the case is totally different.

I continue my quotation:—"Until corrected by Mr. Findlay, Mr. Croll assumed that the whole of the true Gulf-stream continues to flow in a N.E. direction; whereas it is unquestionable that a considerable proportion of it (probably more than one half) turns southwards to the east of the Azores, and reenters the equatorial current" (§ 104).

I am not aware of having advanced any thing which could lead Dr. Carpenter or any one else to suppose that I was of opinion that the whole of the Gulf-stream flows in a N.E. direction, or that I was ignorant of the existence of the S.E. branch. Nor do I remember having seen the correction by Mr. Findlay to which he refers. To suppose that I knew of the existence of the N.E. but not of the S.E. branch, is to assume that I had never seen a chart of the Gulf-stream. If I had seen a chart, how could I possibly have observed the N.E. branch without at the same time perceiving the other? It would be just as possible to look one's friend in the face and notice his left eye without seeing his right.

In reference to these four reasons or arguments designed to show that my figures are valueless, there must be some confusion of ideas. The point to be proved is, that by some wrong method I have been led to form either an erroneous estimate of the quantity of heat conveyed by the Gulf-stream, or the effects resulting from that heat. This is what Dr. Carpenter proposes to do; but no sooner does he make this proposal than he com-

mences to prove something totally different, viz. that there are a great many important causes affecting climate which I entirely ignore or overlook, and that those causes which I ignore or overlook have a far greater influence on climate than the heat of the Gulf-stream. It may be perfectly true that there are a great many important causes affecting climate which I have not considered; and it may likewise be true that those causes, left out of consideration, have a far more powerful influence on climate than the heat conveyed by the Gulf-stream; nevertheless it may be true also that all my statements regarding the influence of the Gulf-stream on climate are perfectly correct. It does not necessarily follow that a horse may not possess a certain amount of strength, and be able to perform a certain amount of work, simply because there are other horses which possess a much greater amount of strength, and can perform a much greater amount of work.

What I have endeavoured to prove in reference to the Gulf-stream is:—that the amount of heat conveyed by it is so enormous as to be equal to one fourth of all the heat received from the sun by the North Atlantic in temperate regions; and that were it not for the Gulf-stream and other ocean-currents, only a very small portion of the globe would be suited to the present orders of sentient beings*—that London, instead of possessing a mean annual temperature of nearly 50° , would have a mean temperature of not over 10° . But I never argued that there were not other causes to which we are far more indebted than to the heat of the Gulf-stream. Were it not for those other causes, the temperature of London would not be simply 40° , but upwards of 500° below what it is at present.

The bearing which my estimate has on Dr. Carpenter's Theory.

There is one point to which I wish to direct special attention, viz. the bearing which my conclusions regarding the quantity of heat conveyed by the Gulf-stream have on Dr. Carpenter's theory of a general interchange of equatorial and polar water. But, in order better to understand this matter, it will be necessary to refer very briefly to a point which has already been discussed at considerable length in former papers. In my earlier paper on the amount of heat conveyed by the Gulf-stream†, I estimated the volume of that stream as *equal to that of a current 50 miles broad and 1000 feet deep, flowing (from the surface to the bottom) at 4 miles an hour.* Of course I did not mean, as Dr. Carpenter seems to suppose, that the stream at any par-

* Phil. Mag. for Feb. 1870.

† Trans. of Geol. Soc. of Glasgow for April 1867; Phil. Mag. for June 1867 (Supplement).

ticular place is 50 miles broad and 1000 feet deep, or that it actually flows at the uniform rate of 4 miles an hour at surface and bottom. All I meant was, that the Gulf-stream is *equal to that* of a current of the above size and velocity. But in my recent papers on Ocean-currents I have taken the volume of the stream at one half this estimate, viz. equal to a current 50 miles broad and 1000 feet deep flowing at the rate of 2 miles an hour. I have estimated the mean temperature of the stream as it passes the Straits of Florida to be 65° , and have supposed that the water in its course becomes ultimately cooled down on an average to 40° *. In this case each pound of water conveys 25 units of heat from the Gulf of Mexico, to be employed in warming temperate and polar regions. Assuming these data to be correct, it follows that the amount of heat transferred from the Gulf of Mexico by this stream per day amounts to 77,479,650,000,000,000 foot-pounds. This enormous quantity of heat is equal to one fourth of all that is received from the sun by the whole of the Atlantic Ocean from the tropic of Cancer up to the Arctic Circle.

This is the amount of heat conveyed from intertropical to temperate and polar regions by the Gulf-stream. What now is the amount conveyed by means of the general oceanic circulation? If this general interchange of equatorial and polar water be, as Dr. Carpenter supposes, the great agency employed in distributing heat over the globe, then surely it is not too much to expect that the quantity of intertropical heat carried into the North Atlantic and Arctic seas must be at least equal to that carried by the Gulf-stream.

If we assume this to be the case, then the combined amount of heat conveyed by the two agencies into the Atlantic from intertropical regions will of course be equal to twice that conveyed by the Gulf-stream alone. Taking the annual quantity of heat received from the sun per unit surface at the equator at 1000, the quantities received by the three zones will be respectively as follows:—

Equator	1000
Torrid zone	975
Temperate zone	757
Frigid zone	454

* It is probable that a large proportion of the water constituting the south-eastern branch of the Gulf-stream is never cooled down to 40° ; but, on the other hand, the north-eastern branch, which passes into the Arctic regions, will be cooled far below 40° , probably below 30° . Hence I cannot be overestimating the extent to which the water of the Gulf-stream is cooled down in fixing upon 40° as the average minimum temperature.

Now it will be seen, by referring to what has been shown on a former occasion (*Phil. Mag.* for Oct. 1870, p. 257), that the Gulf-stream and general oceanic circulation would, in such a case, remove from the torrid zone 405 parts of the 975 received from the sun ; and this transferred to the Atlantic in temperate regions, would add 367 to the 757 already possessed by it. In this case the Atlantic in temperate regions would possess 1126 parts of heat, whereas the intertropical region would possess only 570 parts ; or, in other words, the Atlantic in temperate regions would have twice the amount of heat possessed by it in intertropical regions. But if we assume that one half of this heat goes into the Arctic Ocean, and the other half remains in the temperate regions, the relative quantities of heat possessed by the three zones will be as follows :—

Atlantic, in Torrid zone	570
„ in Temperate zone	940
„ in Frigid zone	766

It is here assumed, however, that none of the heat possessed by the Gulf-stream is derived from the southern hemisphere, which, we know, is not the case. But supposing that as much as one half of the heat possessed by the stream came from the southern hemisphere, and that the other half was obtained from the seas lying between the equator and the tropic of Cancer, the relative proportions of heat possessed by the three zones per given area would be as follows :—

Atlantic, in Torrid zone	671
„ in Temperate zone	940
„ in Frigid zone	766

This proves incontestably that, supposing there is such a circulation as Dr. Carpenter maintains, the quantity of heat conveyed by means of it into the North Atlantic and Arctic Oceans must be trifling in comparison with that conveyed by the Gulf-stream ; for if it nearly equalled that conveyed by the Gulf-stream, then not only the North Atlantic in temperate regions ; but even the Arctic Ocean itself would be much warmer than the intertropical seas. In fact, so far as the distribution of heat over the globe is concerned, it is a matter of indifference whether there really is or is not such a thing as this general oceanic circulation. The enormous amount of heat conveyed by the Gulf-stream alone puts it beyond all doubt that ocean-currents are the great agents employed in distributing over the globe the excess of heat received by the sea in intertropical regions.

It is therefore, so far as concerns the theory of a General Oceanic Circulation, of the utmost importance that the advo-

cates of that theory should prove that I have overestimated the thermal power of the Gulf-stream. This, however, can only be done by detecting some error either in my computation or in the data on which it is based; yet neither Dr. Carpenter nor any one else, as far as I know, has challenged the accuracy of my figures. The question at issue is the correctness of the data; but the only part of the data which can possibly admit of being questioned is my estimate of the *volume* and *temperature* of the stream. Dr. Carpenter, however, does not maintain that I have overestimated the temperature of the stream; on the contrary, he affirms that I have really underestimated it. "If we assume," remarks Dr. Carpenter, "the limit of the stratum above 60° as that of the real Gulf-stream current, we shall find its average temperature to be somewhat higher than it has been stated by Mr. Croll, who seems to have taken 65° as the average of the water flowing through the entire channel. The average surface-temperature of the Florida channel for the whole year is 80° ; and we may fairly set the average of the entire outgoing stream, down to the plane of 60° , at 70° , instead of 65° as estimated by Mr. Croll" (§ 141). It follows, then, that every pound of water of the Gulf-stream actually conveys 5 units of heat more than I have estimated it to do—the amount conveyed being 30 units instead of 25 units as estimated by me. Consequently, if the Gulf-stream be equal to that of a current of merely $41\frac{1}{2}$ miles broad and 1000 feet deep, flowing at the rate of 2 miles an hour, it will still convey the estimated quantity of heat. But this estimate of the volume of the stream, let it be observed, scarcely exceeds *one third* of that given by Herschel, Maury, and Colding (Phil. Mag. Oct. 1871, p. 272), and little more than one half that assigned to it by Mr. Laughton, and but very little exceeds that given by Mr. Findlay*, an author whom few will consider likely to overrate either the volume or heating-power of the stream.

The important results obtained during the 'Challenger' expedition have clearly proved that I have neither overestimated the temperature nor the volume of the Gulf-stream. Between Bermuda and Sandy Hook the stream is 60 miles broad and 600 feet deep, with a maximum velocity of from $3\frac{1}{2}$ to 4 miles an hour. If the mean velocity of the entire section amounts to $2\frac{1}{4}$ miles an hour, which it probably does, the volume of the stream must equal that given in my estimate†. But we have no

* Mr. Findlay considers that the daily discharge does not exceed 333 cubic miles (Brit. Assoc. Rep. 1869, p. 160). My estimate makes it 378 cubic miles. Mr. Laughton's estimate is 630 cubic miles (Paper "On Ocean-currents," Journ. of Royal United-Service Institution, vol. xv.).

† Dr. Carpenter states (§ 140) that 48 miles per day is the mean

evidence that all the water flowing through the Straits of Florida passes through the section examined by the officers of the 'Challenger.' Be this, however, as it may, the observations made between St. Thomas and Sandy Hook reveal the existence of an immense flow of warm water, 2300 feet deep, entirely distinct from the water included in the above section of the Gulf-stream proper. As the thickest portion of this immense body of water joins the warm water of the Gulf-stream, Captain Nares considers that "it is evidently connected with it, and probably as an offshoot." At Sandy Hook, according to him, it extends 1200 feet deeper than the Gulf-stream itself, but off Charleston, 600 miles nearer the source, the same temperature is found at the same depth. But whether it be an offshoot of the Gulf-stream or not, one thing is certain, it can only come from the Gulf of Mexico or from the Caribbean Sea; and that it is a moving stream is proved by the fact that at some places its velocity was found to be as great as 18 miles per day. This mass of water, after flowing northwards for about 1000 miles, turns to the right and crosses the Atlantic in the direction of the Azores, where it appears to thin out.

If, therefore, we take into account the combined heat conveyed by both streams, my estimate of the heat transferred from intertropical regions into the North Atlantic will be found rather under the truth than above it. The quantity of heat thus borne into the Atlantic is enormous compared with that which can possibly be conveyed by a "General Vertical Oceanic Circulation." It follows, therefore, that, so far as the distribution of heat is concerned, it is a matter of perfect indifference whether such a circulation does or does not exist.

THE WIND THEORY OF OCEANIC CIRCULATION.

Ocean-currents not due alone to the Trade-Winds.—The generally received opinion amongst the advocates of the wind theory of oceanic circulation is that the Gulf-stream and other currents of the ocean are due to the impulse of the trade-winds. The tendency of the trade-winds is to impel the intertropical waters along the line of the equator from east to west; and were those regions not occupied in some places by land, this equatorial current would flow directly round the globe. Its westward progress, however, is arrested by the two great continents, the old and the new. On approaching the land the current bifurcates, one

annual rate of the Gulf-stream in the "Narrows;" but in the Admiralty's Current-chart, published October 1872, the minimum rate is stated to be 32, and the maximum rate 120 nautical miles per day. This gives 87 statute miles per day, or fully $3\frac{1}{2}$ miles per hour, as the mean rate.

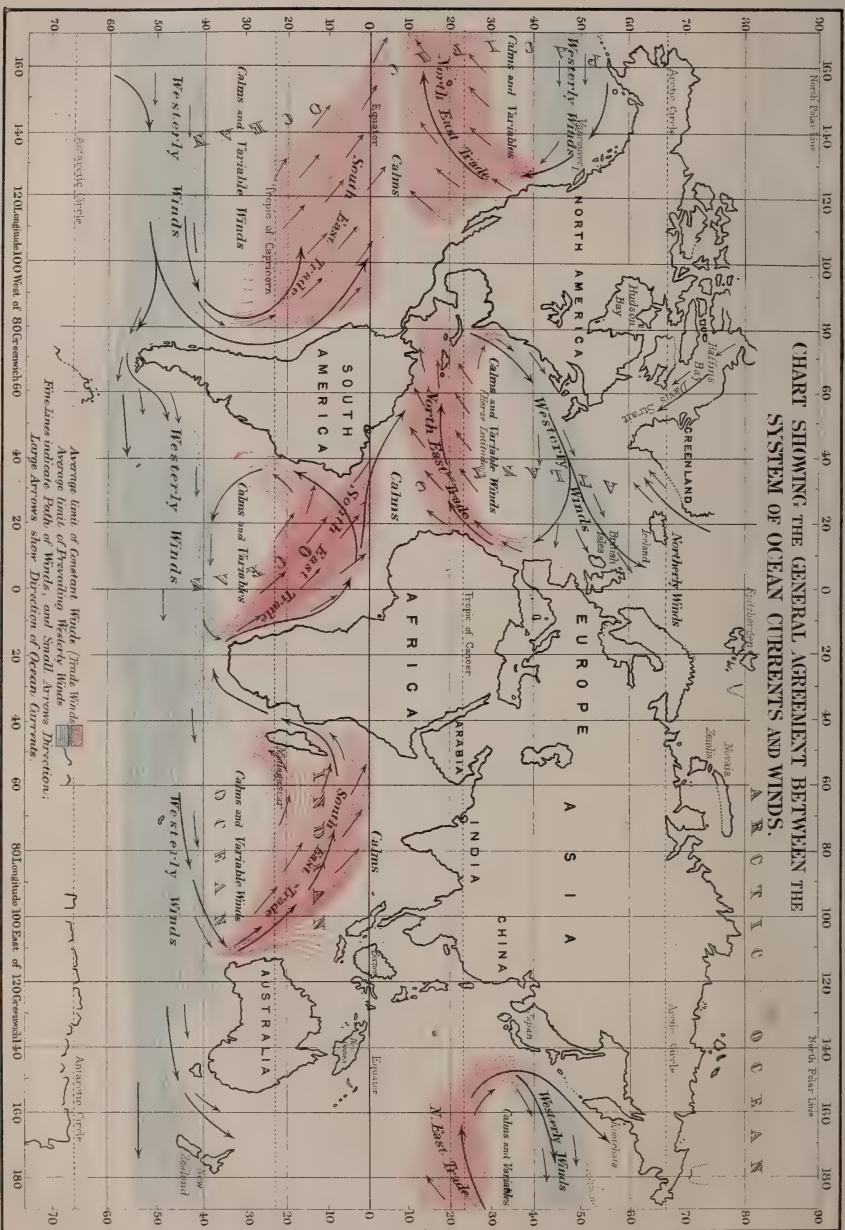
Phil. Mag. S. 4. Vol. 47. No. 311. March 1874. N

portion trending northwards and the other southwards. The northern branch of the equatorial current of the Atlantic passes into the Caribbean Sea, and after making a circuit of the Gulf of Mexico, flows northward and continues its course into the Arctic Ocean. The southern branch, on the other hand, is deflected along the South-American coast, constituting what is known as the Brazilian current. In the Pacific a similar deflection occurs against the Asiatic coast, forming a current somewhat resembling the Gulf-stream, a portion of which (Kamtschatka current) in like manner passes into the Arctic regions. In reference to all these various currents, the impelling cause is supposed to be the force of the trade-winds.

It is, however, urged as an objection by Maury and other advocates of the gravitation theory, that a current like the Gulf-stream, extending as far as the Arctic regions, could not possibly be impelled and maintained by a force acting at the equatorial regions. But this is a somewhat weak objection. It seems to be based upon a misconception of the magnitude of the force in operation. It does not take into account that this force acts on nearly the whole area of the ocean in intertropical regions. If, in a basin of water, say three feet in diameter, a force is applied sufficient to produce a surface-flow one foot broad across the centre of the basin, the water impelled against the side will be deflected to the extremes of the vessel. And this result does not in any way depend upon the size of the basin. The same effect which occurs in a small basin will occur in a large one, provided the proportion between the breadth of the belt of water put in motion and the size of the vessel be the same in both cases. It does not matter, therefore, whether the diameter of the basin be supposed to be three feet, or three thousand miles, or ten thousand miles.

There is a more formidable objection, however, to the theory. The trade-winds will account for the Gulf-stream, Brazil, Japan, Mozambique, and many other currents; but there are currents, such as some of the polar currents, which cannot be so accounted for. Take, for example, the great Antarctic current flowing northward into the Pacific. This current does not bend to the left under the influence of the earth's rotation and continue its course in a north-westerly direction, but actually bends round to the right and flows eastward against the South-American coast, in direct opposition both to the influence of rotation and to the trade-winds. The trade-wind theory, therefore, is insufficient to account for all the facts. But there is yet another explanation, which satisfactorily solves our difficulties. The currents of the ocean owe their origin, not to the trade-winds alone, but to the *prevailing* winds of the globe (including, of course, the trade-winds).

CHART SHOWING THE GENERAL AGREEMENT BETWEEN THE SYSTEM OF OCEAN CURRENTS AND WINDS.



Ocean-currents due to the System of Winds.—If we leave out of account a few small inland sheets of water, the globe may be said to have but one sea, just as it possesses only one atmosphere. We have accustomed ourselves, however, to speak of parts or geographical divisions of the one great ocean, such as the Atlantic and the Pacific, as if they were so many separate oceans. And we have likewise come to regard the currents of the ocean as separate and independent of one another. This notion has no doubt to a considerable extent militated against the acceptance of the theory that the currents are caused by the winds, and not by difference of specific gravity; for it leads to the conclusion that currents in a sea must flow in the direction of the prevailing winds blowing over that particular sea. The proper view of the matter, as I hope to be able to show, is that which regards the various currents merely as members of one grand system of circulation produced, not by the trade-winds alone, nor by the prevailing winds proper alone, but by the combined action of all the prevailing winds of the globe, regarded as one system of circulation.

If the winds be the impelling cause of currents, the *direction* of the currents will depend upon two circumstances, viz.:—(1) the direction of the prevailing winds of the globe, including, of course, under this term the prevailing winds proper and the trade-winds; and (2) the conformation of land and sea. It follows, therefore, that as a current in any given sea is but a member of a general system of circulation, its direction is determined, not alone by the prevailing winds blowing over the sea in question, but by the general system of prevailing winds. It may consequently sometimes happen that the general system of winds may produce a current directly opposite to the prevailing wind blowing over the current. The accompanying Chart (Plate II.) shows how exactly the system of ocean-currents agrees with the system of the prevailing winds. The fine lines indicate the paths of the prevailing winds, and the fine arrows the direction in which the wind blows along those paths. The large arrows show the direction of the principal ocean-currents.

The directions and paths of the prevailing winds have been taken from Messrs. Johnston's small physical Atlas, which, I find, agrees exactly with the direction of the prevailing winds as deduced from the four quarterly wind charts lately published by the Hydrographic Department of the Admiralty. The direction of the ocean-currents has been taken from the Current-chart published by the Admiralty.

In every case, without exception, the direction of the main currents of the globe agrees exactly with the direction of the prevailing winds. There could not possibly be a more convincing

proof that those winds are the cause of the ocean-currents than this general agreement of the two systems as indicated by the chart. Take, for example, the North Atlantic. The Gulf-stream follows exactly the path of the prevailing winds. The Gulf-stream bifurcates in mid Atlantic; so does the wind. The left branch of the stream passes north-eastwards into the Arctic regions, and the right branch south-eastwards by the Azores; so does the wind. The south-eastern branch of the stream, after passing the Canaries, reenters the equatorial current and flows into the Gulf of Mexico; the same, it will be observed, holds true of the wind. A like remarkable agreement exists in reference to all the other leading currents of the ocean. This is particularly seen in the case of the great Antarctic current between long. 140° W. and 160° W. This current, flowing northwards from the Antarctic regions, instead of bending to the left under the influence of rotation, turns to the right when it enters the regions of the westerly winds, and flows eastwards towards the South-American shores. In fact all the currents in this region of strong westerly winds flow in an easterly or north-easterly direction.

Taking into account the effects resulting from the conformation of sea and land, the system of ocean-currents agrees precisely with the system of the winds. All the principal currents of the globe are in fact moving in the exact direction in which they ought to move, assuming the winds to be the sole impelling cause. In short, so perfect is the agreement between the two systems, that, given the system of winds and the conformation of sea and land, and the direction of all the currents of the ocean, or more properly the system of oceanic circulation, might be determined *à priori*. Or given the system of the ocean-currents together with the conformation of sea and land, and the direction of the prevailing winds could also be determined *à priori*. Or, thirdly, given the system of winds and the system of currents, and the conformation of sea and land might be roughly determined. For example, it can be shown by this means that the Antarctic regions are probably occupied by a continent and not by a number of separate islands, nor by sea.

While holding that the currents of the ocean form one system of circulation, we must not be supposed to mean that the various currents are connected end to end, having the same water flowing through them all in succession like that in a heating-apparatus. All that is maintained is simply this, that the currents are so mutually related that any great change in one would modify the conditions of all the others. For example, a great increase or decrease in the easterly flow of Antarctic water in the Southern Ocean would decrease or increase, as the case might be, the strength of the West-Australian current; and this change

would modify the equatorial current of the Indian Ocean, a modification which in like manner would affect the Agulhas current and the Southern Atlantic current—this last leading in turn to a modification of the equatorial current of the Atlantic, and consequently of the Brazilian current and the Gulf-stream. Furthermore, since a current impelled by the winds, as Mr. Laughton in his excellent paper on ocean-currents justly remarks, tends to leave a vacancy behind, it follows that a decrease or increase in the Gulf-stream would affect the equatorial current, the Agulhas current, and all the other currents back to the Antarctic currents. Again, a large modification in the Great Antarctic drift-current would in like manner affect all the currents of the Pacific. On the other hand, any great change in the currents of the Pacific would ultimately affect the currents of the Atlantic and Indian Oceans, through its influence on the Cape-Horn current, the South-Australian current, and the current passing through the Asiatic archipelago; and *vice versâ*, any changes in the currents of the Atlantic or Indian Oceans would modify the currents of the Pacific.

I may now consider the cause of the Gibraltar current. There can be little doubt that this current owes its origin (as Mr. Laughton points out) to the Gulf-stream. "I conceive," that author remarks, "that the Gibraltar current is distinctly a stream formed by easterly drift of the North Atlantic, which, although it forms a southerly current on the coast of Portugal, is still strongly pressed to the eastward and seeks the first escape it can find. So great indeed does this pressure seem to be, that more water is forced through the Straits than the Mediterranean can receive, and a part of it is ejected in reverse currents, some as lateral currents on the surface, some, it appears, as an under current at a considerable depth"*. The funnel-shaped nature of the strait through which the water is impelled helps to explain the existence of the under current. The water being pressed into the narrow neck of the channel tends to produce a slight banking up; and as the pressure urging the water forward is greatest at the surface and diminishes rapidly downwards, the tendency to the restoration of level will cause an underflow towards the Atlantic, because below the surface the water will find the path of least resistance. It is evident indeed that this underflow will not take place toward the Mediterranean, from the fact that that sea is already filled to overflowing by the current received from the outside ocean.

If we examine the Current-chart published by the Hydrographic Department of the Admiralty, we shall find the Gibraltar current represented as merely a continuation of the S.E. flow of Gulf-stream water. Now, if the arrows shown upon this

* Journal of Royal United-Service Institute, vol. xv.

chart indicate correctly the direction of the flow, we must become convinced that the Gulf-stream water cannot possibly avoid passing through the Gibraltar Strait. Of course the excess of evaporation over that of precipitation within the Mediterranean area would of itself produce a considerable current through the Strait; but this of itself would not fill that inland sea to overflowing.

The Atlantic may, in fact, be regarded as an immense whirlpool with the Saragossa Sea as its vortex; and although it is true, as will be seen from an inspection of the Chart, that the wind blows round the Atlantic along the very path taken by the water, impelling the water forward along every inch of its course, yet nevertheless it must hold equally true that the water has a tendency to flow off in a straight line at a tangent to the circular course in which it is moving. But the water is so hemmed in on all sides that it cannot leave this circular path except only at two points; and at these two points it actually does flow outwards. On the east and west sides the land prevents any such outflow. Similarly, in the south the escape of the water is frustrated by the pressure of the opposing currents flowing from that quarter; while in the north it is prevented by the pressure exerted by polar currents from Davis Strait and the Arctic Ocean. But in the Strait of Gibraltar and in the north-eastern portion of the Atlantic between Iceland and the north-eastern shores of Europe there is no resistance offered; and at these two points an outflow does actually take place. In both cases, however, especially the latter, the outflow is greatly aided by the impulse of the prevailing winds.

The consideration that ocean-currents are simply parts of a system of circulation produced by the system of prevailing winds, and not by the impulse of the trade-winds alone, helps to remove the difficulty which some have in accounting for the existence of under currents without referring them to difference of specific gravity. Take the case of the Gulf-stream, which passes under the polar stream on the west of Spitzbergen, this latter stream passing in turn under the Gulf-stream a little beyond Bear Island. The polar streams have their origin in the region of prevailing northerly winds, which no doubt extend to the Pole. The current flowing past the western shores of Spitzbergen, throughout its entire course up to near the point where it disappears under the warm waters of the Gulf-stream, lies in the region of these same northerly winds. Now why should this current cease to be a surface-current as soon as it passes out of the region of northerly into that of south-westerly winds? The explanation seems to be this: when the stream enters the region of prevailing south-westerly winds, its progress southwards along

the surface of the ocean is retarded both by the wind and by the surface-water moving in opposition to its course; but being continually pressed forward by the impulse of the northerly winds acting along its whole course back almost to the pole perhaps, or as far north at least as the sea is not wholly covered with ice, the polar current cannot stop when it enters the region of opposing winds and currents; it must move forward. But the water thus pressed from behind will naturally take the *path of least resistance*. Now in the present case this path will necessarily lie at a considerable distance below the surface. Had the polar stream simply to contend with the Gulf-stream flowing in the opposite direction, it would probably keep the surface and continue its course along the side of that stream; but it is opposed by the winds, from which it cannot escape except by dipping down under the surface; and the depth to which it will descend will depend upon the depth of the surface-current flowing in the opposite direction. There is no necessity for supposing a heaping up of the water in order to produce by pressure a force sufficient to impel the under current. The pressure of the water from behind is of itself enough. The same explanation, of course, applies to the case of the Gulf-stream passing under the polar stream. And if we reflect that these under currents are but parts of the general system of circulation, and that in most cases they are currents compensating for water drained off at some other quarter, we need not wonder at the distance which they may in some cases flow, as, for example, from the banks of Newfoundland to the Gulf of Mexico. The under currents of the Gulf-stream are necessary to compensate for the water impelled southwards by the northerly winds; and again, the polar under currents are necessary to compensate for the water impelled northward by the south and south-westerly winds.

No accurate observations, as far as I know, have been made regarding the amount of work performed by the wind in impelling the water forward; but when we consider the great retarding effect of objects on the earth's surface, it is quite apparent that the amount of work performed on the surface of the ocean must be far greater than is generally supposed. For example, Mr. Buchan, Secretary to the Scottish Meteorological Society, has shown* that a fence made of slabs of wood 3 inches in width and 3 inches apart from each other is a protection even during high winds to objects on the lee side of it, and that a wire screen with meshes about an inch apart affords protection during a gale to flower-pots. The same writer was informed by Mr. Addie that such a screen put up at Rockville was torn to pieces by a

* Paper read to the Edinburgh Botanical Society on January 8, 1874.

storm of wind, the wire screen giving way much in the same way as sails during a hurricane at sea.

Ocean-currents in relation to Change of Climate.—In my attempts to prove that oceanic circulation is produced by the winds and not by difference of specific gravity, and that ocean-currents are the great distributors of heat over the globe, my chief aim has been to show the bearing which these points have on the grand question of secular changes of climate during geological epochs, more particularly in reference to that mystery the cause of the glacial epoch.

In concluding this series of papers, I may therefore be allowed briefly to recapitulate those points connected with the subject of ocean-currents which seem to shed most light on the question of changes of climate, referring the reader for fuller details to former papers on the question.

The complete agreement between the systems of ocean-currents and winds not only shows that the winds are the impelling cause of the currents, but it also indicates to what an extent the *directions* of the currents are determined by the winds, or, more properly, to what an extent their directions are determined by the *direction* of the winds.

We have seen in a former part of this paper (Phil. Mag. Feb. 1870) to what an enormous extent the climatic conditions of the globe are dependent on the distribution of heat effected by means of ocean-currents. It has been there pointed out that, if the heat conveyed from intertropical to temperate and polar regions by oceanic circulation were restored to the former, the equatorial regions would then have a temperature about 55° warmer, and the high polar regions a climate 83° colder than at present. It follows, therefore, that any cause which will greatly affect the currents or greatly change their paths and mode of distribution, will of necessity seriously affect the climatic condition of the globe. But as the existence of these currents depends on the winds, and their direction and form of distribution depend upon the direction and form of distribution of the winds, any cause which will greatly affect the winds will also greatly affect the currents, and consequently will influence the climatic condition of the globe. Again, as the existence of the winds depends mainly on the difference of temperature between equatorial and polar regions, any cause which will greatly affect this difference of temperature will likewise greatly affect the winds; and these will just as surely react on the currents and climatic conditions of the globe. A simple increase or decrease in the difference of temperature between equatorial and polar regions, though it would certainly produce an increase or a decrease, as the case might be, in the strength of the winds, and consequently in the

strength of the currents, would not, however, greatly affect the mode of *distribution* of the winds, and, as a consequence, the mode of *distribution* of the currents. But although a simple change in the difference of temperature between the equator and the poles would not produce a different *distribution* of ærial, and consequently of ocean-currents, nevertheless a *difference in the difference* of temperature between the equator and the two poles would do so; that is to say, any cause that should increase the difference of temperature between the equator and the pole on the one hemisphere, and decrease that difference on the other, would effect a change in the distribution of the ærial currents, which change would in turn produce a corresponding change in the distribution of ocean-currents.

It has been shown* that an increase in the eccentricity of the earth's orbit tends to lower the temperature of the one hemisphere and to raise the temperature of the other. It is true that an increase of eccentricity does not afford more heat to the one hemisphere than to the other; nevertheless it brings about, as I have already shown, a condition of things which tends to lower the temperature of the one hemisphere and to raise the temperature of the other. Let us imagine the eccentricity to be at its superior limit, $\cdot 07775$, and the winter solstice in the aphelion. The midwinter temperature, owing to the increased distance of the sun, would be lowered enormously; and the effect of this would be to cause all the moisture which now falls as rain during winter in temperate regions to fall as snow. Nor is this all; the winters would not merely be colder than now, but they would also be much longer. At present the summer half year exceeds the winter half year by nearly 8 days; but at the period in question the winters would be longer than the summers by upwards of 36 days. The heat of the sun during the short summer, for reasons which have already been explained, would not be sufficient to melt the snow of winter; so that gradually, year by year, the snow would continue to accumulate on the ground.

On the southern hemisphere the opposite condition of things would obtain. Owing to the nearness of the sun during the winter of that hemisphere, the moisture of the air would be precipitated as rain in regions where at present it falls as snow. This and the shortness of the winter would tend to produce a decrease in the quantity of snow. The difference of temperature between the equatorial and the temperate and polar regions would therefore be greater on the northern than on the southern hemisphere; and, as a consequence, the ærial

* Phil. Mag. August 1864, February 1867, March 1870, and other places.

currents of the former hemisphere would be stronger than those of the latter. This would be more especially the case with the trade-winds. The N.E. trades being stronger than the S.E. trades would blow across the equator, and the medial line between them would therefore be at some distance to the south of the equator. Thus the equatorial waters would be impelled more to the southern than to the northern hemisphere; and the warm water carried over in this manner to the southern hemisphere would tend to increase the difference of temperature between the two hemispheres. This change, again, would in turn tend to strengthen the N.E. and to weaken the S.E. trades, and would thus induce a still greater flow of equatorial waters into the southern hemisphere—a result which would still more increase the difference of temperature between the northern and southern hemisphere, and so on—the one cause so reacting on the other as to increase its effects, as was shown at length on a former occasion (Phil. Mag. March 1870).

It was this mutual reaction of those physical agents which led, as I have pointed out (Phil. Mag. March 1870), to that extraordinary condition of climate which prevailed during the glacial epoch.

There is another circumstance to be considered which perhaps more than any thing else would tend to lower the temperature of the one hemisphere and to raise the temperature of the other; and this is the *displacement of the great equatorial current*. During a glacial period in the northern hemisphere the medial line between the trades would be shifted very considerably south of the equator; and the same would necessarily be the case with the great equatorial currents, the only difference being that the equatorial currents, other things being equal, would be deflected further south than the medial line. For the water impelled by the strong N.E. trades would be moving with greater velocity than the waters impelled by the weaker S.E. trades, and, of course, would cross the medial line of the trades before its progress southwards could be arrested by the counteracting influence of the S.E. trades. Let us glance briefly at the results which would follow from such a condition of things. In the first place, as was shown on former occasions (Phil. Mag. for August 1864, February 1867, March 1870), were the equatorial current of the Atlantic (the feeder of the Gulf-stream) shifted considerably south of its present position, it would not bifurcate, as it now does, off Cape St. Roque, owing to the fact that the whole of the waters would strike obliquely against the Brazilian coast and thus be deflected into the Southern Ocean. The effect produced on the climate of the North Atlantic and North-western Europe by the withdrawal of the water forming the Gulf-stream,

may be conceived from what has already been stated concerning the amount of heat conveyed by that stream. The heat thus withdrawn from the North Atlantic would go to raise the temperature of the Southern Ocean and Antarctic regions. A similar result would take place in the Pacific Ocean. Were the equatorial current of that ocean removed greatly to the south of its present position, it would not then impinge and be deflected upon the Asiatic coast, but upon the continent of Australia; and the greater portion of its waters would then pass southward into the Southern Ocean, while that portion passing round the north of Australia (owing to the great strength of the N.E. trades) would rather flow into the Indian Ocean than turn round, as now, along the east coast of Asia by the Japan Islands. The stoppage of the Japan current, combined with the displacement of the equatorial current to the south of the equator, would greatly lower the temperature of the whole of the North Pacific and adjoining continents, and raise to a corresponding degree the temperature of the South Pacific and Southern Ocean. Again, the waters of the equatorial current of the Indian Ocean (owing to the opposing N.E. trades), would not, as at present, find their way round the Cape of Good Hope into the North Atlantic, but would be deflected southwards into the Antarctic sea.

We have in the present state of things a striking example of the extent to which the medial line between the two trades may be shifted, and the position of the great equatorial currents of the ocean may be affected by a slight difference in the relative strength of the two aerial currents. The S.E. trades are at present a little stronger than the N.E.; and the consequence is that they blow across the equator into the northern hemisphere to a distance sometimes of 10 or 15 degrees, so that the mean position of the medial line lies at least 6 or 7 degrees north of the equator.

And it is doubtless owing to the superior strength of the S.E. trades that so much warm water crosses the equator from the South to the North Atlantic, and that the main portion of the equatorial current flows into the Caribbean Sea rather than along the Brazilian coast. Were the two trades of equal strength, the transference of heat into the North Atlantic from the southern hemisphere by means of the Southern Atlantic and Equatorial currents would be much less than at present. The same would also hold true in regard to the Pacific.

Ocean-currents in relation to the Distribution of Plants and Animals.—In the fifth and last editions of the 'Origin of Species,' Mr. Darwin has done me the honour to express his belief that the foregoing view regarding alternate cold and warm periods

in north and south during the glacial epoch explains a great many facts in connexion with the distribution of plants and animals which have always been regarded as exceedingly puzzling.

There are certain species of plants which occur alike in the temperate regions of the southern and northern hemispheres. At the equator these same temperate forms are found on elevated mountains, but not on the lowlands. How, then, did these temperate forms manage to cross the equator from the northern temperate regions to the southern, and *vice versa*? Mr. Darwin's solution of the problem is (in his own words) as follows:—

“As the cold became more and more intense, we know that Arctic forms invaded the temperate regions; and from the facts just given, there can hardly be a doubt that some of the more vigorous, dominant, and widest-spreading temperate forms invaded the equatorial lowlands. The inhabitants of these hot lowlands would at the same time have migrated to the tropical and subtropical regions of the south; for the southern hemisphere was at this period warmer. On the decline of the Glacial period, as both hemispheres gradually recovered their former temperatures, the northern temperate forms living on the lowlands under the equator would have been driven to their former homes or have been destroyed, being replaced by the equatorial forms returning from the south. Some, however, of the northern temperate forms would almost certainly have ascended any adjoining high land, where, if sufficiently lofty, they would have long survived like the Arctic forms on the mountains of Europe.”

“In the regular course of events the southern hemisphere would in its turn be subjected to a severe glacial period, with the northern hemisphere rendered warmer; and then the southern temperate forms would invade the equatorial lowlands. The northern forms which had before been left on the mountains would now descend and mingle with the southern forms. These latter, when the warmth returned, would return to their former homes, leaving some few species on the mountains, and carrying southward with them some of the northern temperate forms which had descended from their mountain fastnesses. Thus we should have some few species identically the same in the northern and southern temperate zones and on the mountains of the intermediate tropical regions.” (P. 339, sixth edition.)

Additional light is cast on this subject by the results already stated in regard to the enormous extent to which the temperature of the equator is affected by ocean-currents. Were there no transference of heat from equatorial to temperate and polar regions, the temperature of the equator, as has been remarked, would probably be about 55 degrees warmer than at present. In such a case no plant existing on the face of the globe could live

at the equator unless on some elevated mountain-region. On the other hand, were the quantity of warm water which is being transferred from the equator to be very much increased, the temperature of intertropical latitudes might be so lowered as easily to admit of temperate species of plants growing at the equator. A lowering of the temperature at the equator some 20 or 30 degrees is all that would be required; and only a moderate increase in the volume of the currents proceeding from the equator, taken in connexion with the effects flowing from the following considerations, might suffice to produce that result. During the Glacial epoch, when the one hemisphere was under ice and the other enjoying a warm and equable climate, the medial line between the trades may have been shifted to almost the tropical line of the warm hemisphere. Under such a condition of things the warmest part would probably be somewhere about the tropic of the warm hemisphere, and not, as now, at the equator; for since all, or nearly all, the surface-water of the equator would then be impelled over to the warm hemisphere, the tropical regions of that hemisphere would be receiving nearly double their present amount of warm water.

Again, as the equatorial current at this time would be shifted towards the tropic of the warm hemisphere, the surface-water would not, as at present, be flowing in equatorial regions parallel to the equator, but obliquely across it from the cold to the warm hemisphere. This of itself would tend greatly to lower the temperature of the equator.

It follows, therefore, as a necessary consequence, that during the glacial epoch, when the one hemisphere was under snow and ice and the other enjoying a warm and equable climate, the temperature of the equator would be lower than at present. But when the glaciated hemisphere (which we may assume to be the northern) began to grow warmer and the climate of the southern or warm hemisphere to get colder, the medial line of the trades and the equatorial currents of the ocean also would begin to move back from the southern tropic towards the equator. This would cause the temperature of the equator to rise and to continue rising until the equatorial currents reached their normal position. When the snow began to accumulate on the southern hemisphere and to disappear on the northern, the medial line of the trades and the equatorial currents of the ocean would then begin to move towards the northern tropic as they had formerly towards the southern. The temperature of the equator would then again begin to sink, and continue to do so until the glaciation of the southern hemisphere reached its maximum. This oscillation of the line of maximum temperature to and fro across

Comparing expressions (I.) and (II.), it appears that they differ only by the factor $2enR$. And if now we adopt the assumption, certainly favoured by many physicists, that e in the same portion of the conductor is for all currents constantly the same, and therefore the action of different currents in such portion differs only by the various values of v , it will be seen that, in the same conductor, expression (I.) is for all currents the same multiple of expression (II.).

There is, then, only necessary the knowledge of a number which (as one may perhaps dare to say) specifies how often in the unit of time the *vis viva* of the electric motion (constantly transformed into heat) is reproduced by the electromotive forces, in order from one value of the *vis viva* to deduce at once the total thermal effect of the current. Perhaps the following consideration may elucidate this number.

Let us imagine from any place in the conductor the resistance and consequently its thermal effect to cease; from that moment the space of the conductor must be much more filled only with electric fluid, and, indeed, at first of the same density as in the conductor. The electric motion would then be propagated in this fluid without enfeeblement. If we call the constant velocity of propagation of the motion in this case, expressed in millimetres, C , in 1 second a *vis viva* certainly not less than the quantity $\frac{Ce}{n} \cdot \frac{v^2}{2}$ would arise*.

As starting-point of this entire motion a layer of the conductor of the length $\frac{1}{\sqrt[3]{M}}$ is sufficient, if M denotes the number of molecules of the electric fluid contained in 1 cubic millim. of the conductor, understanding by "molecule" just so much fluid that a translatory motion of it of the sort supposed can be originated.

And now, according to the preceding, the value $\frac{Ce}{n} \cdot \frac{v^2}{2}$ may very well be supposed equal to the thermal effect actually accomplished in the same time, with the action of the resistance, by the layer $\frac{1}{\sqrt[3]{M}}$. We should then have

$$e^2 v^2 R \cdot \frac{1}{\sqrt[3]{M}} = \frac{Ce}{n} \cdot \frac{v^2}{2} \cdot 9811,$$

where the factor 9811 is put because in (I.) the acceleration of

* It has been already remarked that no account need be taken of further factors in this expression, which at all events would be requisite if oscillations were thought of.

1 millim. (on which the electrostatic measure is founded) is presupposed, and in (II.), on the contrary, the acceleration in a free fall (which forms the basis of the calculation with the usual weights).

If the conductor has a cross section of the unit of surface, $\frac{e}{n}$ is simply the density of the fluid reckoned in ordinary weights. Let it be put equal to D. Then would

$$\sqrt[3]{M} = \frac{2e^2R}{9811 \cdot C \cdot D} \cdot \cdot \cdot \cdot \cdot \cdot \quad (\text{III.})$$

Now this relation would attain a very much further-reaching practical signification if, in connexion with hypotheses which, especially recently, have been repeatedly mooted, by M were to be understood the number of æther molecules contained in a cubic millimetre of the conductor, and by C the velocity of propagation of light in the free æther. Then would D be at all events much greater than the density of the æther in free space. Sir

W. Thomson* has calculated that the latter is greater than $\frac{1}{10^{22}}$;

Belli†, indeed, has subsequently stated it to be at least $\frac{1}{21 \cdot 10^{12}}$.

As, unfortunately, I have not been able to examine Belli's calculation, while the value given by Thomson is certainly very much too small, $\frac{1}{10^{18}}$ may perhaps be taken, as lying between the two.

We should then have, understanding by M and C the above-stated quantities, the inequality

$$\sqrt[3]{M} < \frac{2e^2R \cdot 10^{18}}{9811 \cdot 3 \cdot 10^{11}} \text{ or } < \frac{e^2R \cdot 10^5}{147}; \quad \cdot \cdot \cdot \quad (\text{IV.})$$

and the inequality hereby expressed would indeed be considerable.

Now R is known for a definite conductor; and so a maximum value of M is given by the preceding relation, in case e is known—or, conversely, a minimum value of e for a known M. It is true, indeed, that both M and e are unknown to us; but we have more closely approximate ideas concerning e than concerning M, and can therefore make use of the relation we have obtained in order to gain similar notions with respect to M.

If, e. g., for mercury we assume $e = 10^{20}$ nearly, remembering that, on the hypothesis of the equality of the velocity of elec-

* Phil. Mag. S. 4. vol. ix. p. 36.

† Cf. *Fortschritte der Physik*, 1859.

trolytic motion to our electrical v , e has been found, according to Weber*, to be about $3 \cdot 10^{13}$ for 1 cubic millim. of water, even this value will perhaps be sufficiently great.

For Siemens's mercury unit, according to Kohlrausch† the resistance is, in electromagnetic measure, $9717 \cdot 10^6$; according

to Weber‡, therefore, it is about $= \frac{1}{24 \cdot 10^{11}}$ in mechanical mea-

sure. Consequently for 1 cubic millim. of mercury, $R = \frac{1}{24 \cdot 10^{14}}$.

Accordingly, in this volume of mercury we should have

$$\sqrt[3]{M} < \frac{10^{29}}{35},$$

or $M < 2 \cdot 10^{82}$ nearly §.

If the number above admitted for the density of the free æther be divided by this value, we get a lower limit for the weight (in milligr.) of each molecule of æther. It would thus amount to considerably more than $\frac{1}{10^{100}}$.

These values have of course only a widely approximate signification, since we have no very exact knowledge of the numerical values of e ; nevertheless it would be of high importance to have, only so far, at least an idea of the relations of the æther.

Aachen, October 26, 1873.

XXV. *On the Manufacture and Theory of Diffraction-gratings.*

By LORD RAYLEIGH, M.A., F.R.S.

[Concluded from p. 93.]

THE remainder of this paper is principally occupied with theoretical considerations relating to the performance of gratings considered as light-analyzing apparatus. The more popular works on the theory of light give only the main outlines of the subject, and pass over almost in silence the important questions of illumination and definition. On the other hand, the mathematical treatises, such as Airy's 'Tracts' and Verdet's *Leçons*, though they give analytical results involving most of the required information, are occupied rather with explaining the production of spectra as a diffraction-phenomenon than with investigating on what conditions their perfection depends. On

* *Electrodynamische Maassbestimmungen*, 1856, p. 281.

† *Pogg. Ann. Ergänz.* vol. vi. p. 1.

‡ *Electrodynamische Maassbestimmungen*, p. 260.

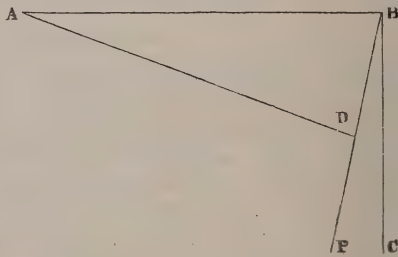
§ Compare W. Thomson's calculation (*Liebig's Ann.* vol. clvii. p. 54), from which the number of physical molecules in the same volume would be, at most, 10^{23} .

examining the question for myself, I came to the conclusion that the theory of gratings, as usually presented, is encumbered with a good deal that may properly be regarded as extraneous.

One of the first things to be noticed is the extraordinary precision required in the ruling. The difference of wave-length of the two sodium-lines is about a thousandth part. If, therefore, we suppose that one grating has 1000 lines in the space where another has 1001, it is evident that the first grating would produce the same deviation for the less-refrangible D line that the second would produce for the more-refrangible D line. We have only to suppose the two combined into one in order to see that, in a grating required to resolve the D line, there must be no systematic irregularity to the extent of a thousandth part of the small interval. Single lines may, of course, be out of position to a much larger amount. It is easy to see, too, that the same accuracy is required, whatever be the order of the spectrum examined.

The precision of ruling actually attained in gratings is very great. In the 3000 Nobert it is certain that the average interval between the lines does not vary by a six-thousandth part in passing from one half of the grating to the other; for the D lines, when well defined, do not appear so broad as a sixth part of the space separating them.

In considering the influence of the number of lines (n) and the order of the spectrum (m), we will suppose that the ruling is accurate, and that plane waves are incident perpendicularly upon the face of the grating whose width is represented in the figure by A B. But inasmuch as a large part of the phenomenon covered by the usual mathematical investigation depends upon the *limitation* of the grating at A and B, we shall find it convenient to take first the simple case of an aperture represented by A B, and afterwards to consider the influence of the ruling.



In the perpendicular direction B C all the secondary waves emanating from A B are in complete agreement of phase, and their resultant accordingly attains its highest possible value. In a direction B P, making with B C a very small angle, the agreement of phase will be disturbed. If B P be so drawn that the projection of A B upon it is equal to λ , the phases of the secondary waves will be distributed uniformly over a complete period,

and the resultant will therefore be *nil*. The same result must ensue whenever BD is an exact multiple of λ .

For the intermediate directions we require a little more calculation.

The phase of the resultant will always correspond with that of the secondary wave which issues from the middle of the aperture. If x denotes the retardation of any element with respect to this one, the amplitude of the resultant is given by

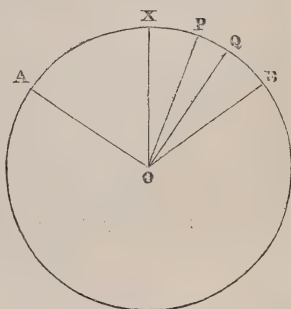
$$\int_{-\frac{R}{2}}^{+\frac{R}{2}} \cos x \, dx \div R,$$

where R is the relative retardation of the extreme parts A and B , or, on integration,

$$\sin \frac{R}{2} \div \frac{R}{2}.$$

This expression gives the magnitude of the resultant amplitude compared with that in the principal direction BC , where all the components agree in phase.

The composition of elementary vibrations whose phases vary uniformly within certain limits may be illustrated by a mechanical analogy. Each elementary vibration is represented by a force proportional to the element of circular arc PQ and acting at O along a direction OP , making with a fixed line of reference OX an angle corresponding to the phase of the vibration. The force may be supposed to be due to the attraction of the arc on a particle placed at O . The group of vibrations is thus represented by the group of forces whose directions are distributed uniformly through the angle AOB ; and the resultant of the forces, found by resolving in the ordinary way, represents on the same system the resultant of the vibrations. In the present case AOB corresponds to R , and the integrated expression $\sin \frac{R}{2} \div \frac{R}{2}$ denotes the ratio of the resultant force to the aggregate of its components calculated without allowance for the difference of direction—that is, as if the whole attracting mass were concentrated at X .



According to what has been already explained, $\left(\sin \frac{R}{2} \div \frac{R}{2} \right)^2$

vanishes when $\frac{R}{2}$ is an even multiple of $\frac{\pi}{2}$. The positions of the maxima (determined by $\tan \frac{R}{2} = \frac{R}{2}$) do not exactly bisect the angles between the vanishing directions; but it will be sufficient for our present purpose to note that the principal maximum ($R=0$) is unity, and that the others do not differ greatly from $\left(\frac{2}{3\pi}\right)^2$, $\left(\frac{2}{5\pi}\right)^2$, $\left(\frac{2}{7\pi}\right)^2$, &c. It is evident that on either side of the principal direction the illumination falls off with great rapidity. If AB is 1 (inch) and $\lambda = \frac{1}{40,000}$, the angle CBP corresponding to the first minimum is only about $5''$.

The image of an infinitely narrow line of light (whose length is perpendicular to the plane of the diagram), as formed by an object-glass with aperture AB , is thus a series of parallel stripes, composed of a central narrow band whose illumination varies from a maximum in the middle to zero at the edges, enclosed by parallel bands of rapidly decreasing illumination. We have now to examine the effect of the ruling.

For the sake of simplicity, we will take first the case of a grating composed of transparent bars of width a , alternating with opaque bars of width d , and consider the central image or spectrum of zero order. In the principal direction, BC , the secondary waves are, as before, in complete agreement, but the amplitude is diminished by the ruling in the ratio $a : a + d$. In another direction, making a small angle with BC such that the relative retardation of A and B amounts to a few wave-lengths, it is easy to see that the mode of interference is the same as if there were no ruling. For example, when the direction is such that the projection of AB upon it amounts to one wave-length, the elementary components neutralize one another, because their phases are on the whole distributed symmetrically, though discontinuously, round the entire circumference. The only effect of the ruling is to diminish the amplitude in the ratio $a : a + d$; and except for the difference in illumination, the appearance of a line of light is exactly the same as if the aperture were perfectly free.

The lateral images occur in such directions that the projection of the element $a + d$ of the grating upon them is an exact multiple of λ . The effect of each element of the grating is then the same; and unless this vanishes on account of a particular adjustment of the ratio $a : d$, the resultant amplitude becomes comparatively very great. These directions, in which the retardation between A and B is exactly $m\lambda$, may be called the principal directions. On either side of any one of them the illumi-

nation is distributed according to the same law as for the central image ($m=0$), vanishing, for example, when the retardation amounts to $(mn \pm 1)\lambda$. In considering the relative brightness of the different spectra, it is therefore sufficient to attend merely to the principal directions, provided that the whole deviation be not so great that its cosine differs considerably from unity*.

Under the restriction just stated, the intensity of the secondary waves may be supposed not to be diminished by the obliquity; and thus we obtain for the ratio of brightness:—

$$B_m : B_0 = \left[\int_{-\frac{am\pi}{a+d}}^{+\frac{am\pi}{a+d}} \cos x \, dx \div \frac{2am\pi}{a+d} \right]^2 = \left(\frac{a+d}{am\pi} \right)^2 \sin^2 \frac{am\pi}{a+d};$$

where B_m denotes the brightness of the m th spectrum, and B_0 of the central image.

If B denotes the brightness of the central image when the whole of the space occupied by the grating is transparent, we have

$$B_0 : B = a^2 : (a+d)^2,$$

and thus

$$B_m : B = \frac{1}{m^2 \pi^2} \sin^2 \frac{am\pi}{a+d}.$$

The sine of an angle can never be greater than unity; and consequently under the most favourable circumstances only $\frac{1}{m^2 \pi^2}$ of the original light can be obtained in the m th spectrum. We conclude that, with a grating composed of transparent and opaque parts, the utmost light obtainable in any one spectrum is in the first (the central image not being included), and there amounts to $\frac{1}{\pi^2}$, or about $\frac{1}{10}$, and that for this purpose a and d must be equal.

When $d=a$, the general formula becomes

$$B_m : B = \frac{1}{m^2 \pi^2} \sin^2 m \frac{\pi}{2},$$

showing that, when m is even, B_m vanishes, and that, when m is odd, $B_m : B = \frac{1}{m^2 \pi^2}$.

The third spectrum has thus only one ninth of the brilliancy of the first.

It is here supposed that the light is homogeneous; but if it

* This point is perhaps made clearer by supposing the original light to be always incident at such an angle that the diffracted spectrum under consideration occurs in the normal direction.

be composed of elements of continuously varying wave-length, another factor, $\frac{1}{m}$, must be introduced, due to the varying elongation of the spectra. The deficiency of light accompanying an increased dispersion, however, is nothing peculiar to gratings, and may be met by a proportional widening of the slit, without lowering the original standard of purity.

Another particular case of interest is obtained by considering a small relatively to $a+d$. Unless the spectrum be of high order, we have simply

$$B_m : B = \left(\frac{a}{a+d} \right)^2,$$

so that the brightness of all the (lateral) spectra is the same. According to this, the spectra of a sodium-flame observed through a grating should be of equal brilliancy when a is small relatively to $a+d$. In the grating with 3000 lines this condition is fulfilled; but the theoretical result is contradicted by observation, the second and third lateral spectra being much brighter than the first. I am not in a position to explain the discrepancy, which I only noticed while drawing up this paper for the press. Unless due to some mathematical blunder, the cause would appear to be deep-seated. The effect of an insufficient narrowness of grooves would be in the opposite direction.

Our expressions have been obtained without taking into account the reflection from the face of the grating; and therefore the light stopped by the opaque parts, together with that distributed in the central image and lateral spectra, ought to make up the original brightness. Thus, if $a=d$, we ought to have

$$\frac{1}{2} = \frac{1}{4} + \frac{2}{\pi^2} \left(1 + \frac{1}{9} + \frac{1}{25} + \dots \right),$$

which is true by a known theorem. In the general case,

$$\frac{a}{a+d} = \left(\frac{a}{a+d} \right)^2 + \frac{2}{\pi^2} \sum_{m=1}^{m=\infty} \frac{1}{m^2} \sin^2 \left(\frac{m\pi a}{a+d} \right),$$

a result which may be verified by Fourier's theorem.

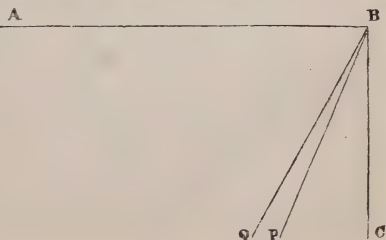
By Babinet's principle it is a matter of indifference, so far as the brightness of the lateral spectra are concerned, whether a represent the width of the transparent or of the opaque part, inasmuch as the secondary waves from the transparent and opaque parts together would give a zero resultant. The same conclusion may be derived from the expression for the ratio $B_m : B$.

From the value of $B_m : B_0$ we see that no lateral spectrum can surpass the central image in brightness; but this result depends upon the hypothesis that the lines of the grating act by opacity,

which is generally very far from being true. In an engraved glass grating or in a gelatine copy there is no opaque material present by which light could be absorbed, and the effect depends on a difference of retardation due to the alternate parts. It is remarkable that this point is never alluded to in the ordinary treatises on optics, and, so far as I know, was first noticed by Quincke (Pogg. *Ann.* vol. cxxxii. p. 321 (1867)), who made a theoretical and experimental examination of the phenomena presented when light is diffracted at the edge of a transparent obstacle. My attention was first drawn to it, before I was acquainted with Quincke's work, by observing that, contrary to my anticipations, it was possible for the lateral spectra of a soda-flame to exceed the central image in brilliancy. When once the question is raised, the explanation is easy enough; for if the grating were composed of equal alternate parts, both alike transparent but giving a relative retardation of half a wave-length, it is evident that the central image would be entirely extinguished, while the first spectrum would be four times as bright as if the alternate parts were opaque. If it were possible to introduce at every part of the aperture of the grating an arbitrary retardation, *all* the light might be concentrated in any desired spectrum. By supposing the retardation to vary uniformly and continuously, we fall on the case of the ordinary prism; but there would then be no analysis of light, except such as depends on the variation of retardation with wave-length. To obtain a diffraction-spectrum in the ordinary sense containing all the light, it would be necessary that the retardation should gradually alter by a wave-length in passing over each element of the grating and then fall back to its previous value, thus springing suddenly over a wave-length. It is not likely that such a result will ever be obtained in practice; but the case was worth stating, in order to show that there is no theoretical limit to the concentration of light of assigned wave-length in one spectrum, and as illustrating the frequently observed unsymmetrical character of the spectra on either side of the central image.

We have now to consider the dependence of the *resolving-power* of a grating on the number of its lines (n) and the order of the spectrum observed (m).

Let BP be the direction of the principal maximum for the wave-length λ in the m th spectrum; then the projection of AB on BP is $mn\lambda$. If BQ be the direction corresponding to the first minimum, the pro-



jection of A B on B Q is $(mn + 1)\lambda$. Suppose now that $\lambda + \delta\lambda$ is the wave-length for which B Q gives the principal maximum, then

$$(mn + 1)\lambda = mn(\lambda + \delta\lambda);$$

whence

$$\frac{\delta\lambda}{\lambda} = \frac{1}{mn},$$

which shows that the resolving-power varies directly as m and n .

It is not possible to say precisely under what circumstances a double line would appear to be resolved—something no doubt would depend on the intensity of the light—but it seems probable that there would be no distinct resolution when the two images are separated by only half the width of the central band of either. If this be so, we may take $\frac{\lambda}{m\delta\lambda}$ as the least number of grooves capable of resolving in the m th spectrum a double line whose wave-lengths are λ and $\lambda + \delta\lambda$. In the case of the soda-lines $\frac{\delta\lambda}{\lambda}$ is about $\frac{1}{1000}$; and therefore to resolve them in the first spectrum would require 1000 grooves, in the second spectrum 500, and so on. It is evident that if the ruling be perfectly accurate and the illumination sufficient, the work may be accomplished with comparatively few lines by using a spectrum of elevated order.

The result of an attempt to determine experimentally the number of lines necessary to resolve D in the solar spectrum may here be recorded. The 3000-to-the-inch Nobert was used, its horizontal aperture being limited by the jaws of an adjustable slit. From the width of the slit found to be necessary the number of lines in operation was calculated.

In the first spectrum 1200 lines were required, in the second 630, and in the third 375. These numbers should be in the ratio 6 : 3 : 2; but the last, which presents the greatest deviation, was difficult of exact measurement.

The number of lines necessary in the first spectrum is very much what might have been expected. The effect of a limitation of the aperture of the grating by a slit whose length is *horizontal*, is, of course, quite different. As the slit is narrowed, the image of a *point* would be dilated in a vertical direction; but this is of no moment when the subject of observation is itself a uniform vertical line. As was mentioned in the first part, the definition is often materially improved.

Since a grating resolves in proportion to the total number of its grooves, it might be supposed that the defining-power depends on different principles in the case of gratings and prisms;

but the distinction is not fundamental. The limit to definition arises in both cases from the impossibility of representing a line of light otherwise than by a band of finite though narrow width, the width in both cases depending only on the horizontal aperture (for a given λ). If a grating and a prism have the same horizontal aperture and dispersion, they will have equal resolving-powers on the spectrum; the greater dispersion is the only cause of the superiority of the diffraction-spectra of high order.

In estimating the value of light-analyzing apparatus, there are three things to be considered—the brightness, the purity, and the apparent magnitude of the resulting spectrum. In the case of a prism, where the loss of light by reflection and absorption may in a rough approximation be neglected, the first two characteristics are inseparably connected, whether a telescope be used or not, so long as the pupil of the eye is filled with light. In whatever degree the purity be enhanced, whether by increasing the dispersion or narrowing the slit, in the same degree must the brightness suffer. The angular magnitude of the spectrum is merely a question of magnifying-power. No matter how small the dispersion may be, the spectrum may yet be made to appear as long as we please by sufficiently increasing the focal length of the object-glass and the power of the eyepiece. But if the brilliancy is not to suffer, the size of the prism and the aperture of the telescope must be proportionally increased; for otherwise the condition will be violated of keeping the pupil filled with light. There are thus two ways of obtaining a powerful spectroscope. The first is to procure a great dispersion by multiplication of prisms; the second is to be satisfied with a small dispersion, and attain the necessary length of spectrum by a high-power telescope, which may involve a larger aperture. It may be questioned whether the second method has received as much attention as it deserves. When there is light to spare, a higher power than usual may often be employed advantageously without an augmentation of aperture.

In the case of gratings the question is complicated by the choice of spectra; but some remarks may perhaps be useful. Much misapprehension appears to exist as to the nature of the advantage derived from close ruling. It is generally supposed that the closer the ruling the greater the resolving-power of the instrument; but this is for the most part a mistake. When there is no limitation on the order of the spectrum to be observed, resolving-power depends not on the *closeness* but on the *accuracy* of the ruling. Let us take the case of a grating with 3000 lines in an inch, and consider the effect of interpolating an additional 3000 lines. The effect of the addition will be to destroy by interference the first, third, and the odd spectra generally; while the advan-

tage gained in the spectra of even order is not in dispersion, nor in resolving-power, but simply in brilliancy, which is increased to four times. If we now suppose half the grating to be cut away, so as to leave 3000 lines in half an inch, the dispersion will evidently not be altered, while the brightness and resolving-power are halved. If, therefore, resolving-power be our object, we should aim at covering a considerable breadth with very accurately placed lines, rather than at extreme closeness of the lines themselves. On the other hand, for experiment on dark heat, or whenever a narrow slit is not available, resolving-power is of less importance, and the best grating will be one that covers the largest space with the finest lines.

I have already mentioned that my 6000-to-the-inch Nobert defines not only not better, but decidedly worse, than the one with 3000 lines in the inch. This inferior definition is probably an accident; for there seems to be no theoretical reason for it. In brightness the closer-ruled grating has greatly the advantage.

The preceding investigations are founded on the principles ordinarily adopted in explaining diffraction-phenomena, and not on a strict dynamical theory. In the present state of our knowledge with respect to the nature of light and its relations to ponderable matter, vagueness in the fundamental hypotheses is rather an advantage than otherwise; a precise theory is almost sure to be wrong. Nevertheless it is often instructive to examine optical questions from a more special point of view; and therefore I hope that an investigation of an ideal grating on dynamical principles will not be out of place, though not very closely connected with the preceding portion of the paper.

In actual gratings the lines or grooves occur at the boundary of two media of different refrangibilities; but, for the sake of simplicity, we shall here suppose the medium on both sides to be the same. The grating will thus consist simply of bars (infinitely long) whose optical properties differ from those of the rest of the medium; and we further suppose

(1) that the variation of optical properties depends upon a difference of inertia, and is small in amount;

(2) that the diameter of the bars is very small in relation to the wave-length of light.

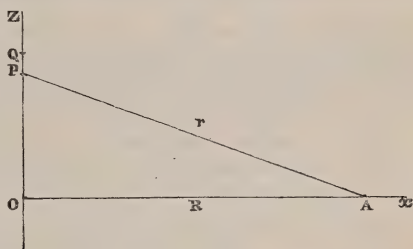
The supposition that refraction depends upon a difference of inertia is that of Fresnel and Green, and has been shown by the latter to lead to Fresnel's laws. In several papers in this Magazine*, I have shown that, if the analogy with an elastic solid holds good at all, no other supposition is reconcilable with the facts of the reflection of light from surfaces and its diffraction

* Phil. Mag. 1871, February, April, June, August.

by small particles. Whether true or not, it is at any rate mechanically possible.

Since the bars are very small, the effect of each is quite independent of the rest; and so the dynamical theory need only concern itself with one. In my paper "On the Light from the Sky"*, I proved that the effect of a body in disturbing the waves of light incident upon it may be calculated by ordinary integration from those of its parts, provided that the square of the alteration of mechanical properties may be neglected. This proposition, though true as stated, requires some caution in use, and is practically inapplicable when the body is elongated in the direction of original propagation, because the dimension of the body in this direction divided by λ may occur as a factor in the terms omitted. In the present case, however, where the light is incident normally to the plane of the grating, this difficulty does not arise.

Let the bar under consideration be taken for axis of z , and let the axis of x be parallel to the direction of propagation of the original light. The original vibration is thus, according to the polarization, parallel to either z or y . We will take first the former case, where the disturbance due to the bar



must be symmetrical in all directions round OZ , and parallel to it. The element of the disturbance at A due to PQ (dz) will be proportional to dz in amplitude, and will be retarded in phase by an amount corresponding to the distance r . In calculating the effect of the whole bar, we have to consider the integral

$$\int_0^\infty \frac{dz}{r} \cos \frac{2\pi}{\lambda} (bt-r) = \int_R^\infty \frac{dr \cos \frac{2\pi}{\lambda} (bt-r)}{\sqrt{r^2 - R^2}}.$$

Now the denominator $= \sqrt{r-R}\sqrt{r+R}$, a product of which the second factor may be treated as constant in the integration in view of the fact that the parts for which r differs much from R destroy each other's effects. After this simplification the integral may be evaluated by means of the formula

$$\int_0^\infty \frac{\sin x}{\sqrt{x}} dx = \int_0^\infty \frac{\cos x}{\sqrt{x}} dx = \sqrt{\frac{\pi}{2}}.$$

* Phil. Mag. February 1871.

The result is

$$\int_0^\infty \frac{dz}{r} \cos \frac{2\pi}{\lambda} (bt-r) = \frac{\sqrt{\lambda}}{2\sqrt{R}} \cos \frac{2\pi}{\lambda} \left(bt-R-\frac{\lambda}{8} \right),$$

showing that the total effect is retarded $\frac{\lambda}{8}$ behind that due to the

element at O. This result is analogous to, though different from, that of the ordinary integration by Huyghens's zones. In that case the effect of each zone is very nearly the same, and therefore the whole is the half of that of the first zone. If the first zone be divided into rings by circles drawn so that r increases in arithmetical progression, the rings will be of equal area, and therefore the phase of the resultant vibration will be halfway between that corresponding to the centre and circumference—that is, will be retarded relatively to the centre by one *fourth* of λ . In the present case, if the bar be divided on the same principle so that each piece gives a result retarded $\frac{\lambda}{2}$ behind its predecessor, the lengths will rapidly diminish from the centre outwards, and therefore the same argument does not apply. The retardation of the resultant relatively to the central element is less than before, on account of the preponderance of the central parts.

By the result investigated in my paper previously referred to, if T be the volume of the element P Q, D and D' the original and altered densities, the disturbance at A due to the element is

$$\frac{D'-D}{D} \frac{\pi \cdot T}{r\lambda^2} \sin \alpha \cos \frac{2\pi}{\lambda} (bt-r)^*,$$

the original vibration at P Q being denoted by $\cos \frac{2\pi}{\lambda} bt$. α is

the angle between the ray PA and the direction of original vibration O Z; but in the present application we may put $\sin \alpha = 1$, since only the central parts are really operative. If we replace T by $A dz$, A being the sectional area of the bar, and use the integral just investigated, we find for the effect of the whole bar

$$\frac{D'-D}{D} \frac{A\pi}{\lambda^2} \sqrt{\frac{\lambda}{R}} \cos \frac{2\pi}{\lambda} \left(bt-R-\frac{\lambda}{8} \right),$$

corresponding to $\cos \frac{2\pi}{\lambda} bt$ for the incident light.

When the original vibration is parallel to y , the disturbance due to the bar will no longer be symmetrical round O Z. If α be the angle between O x and the direction of the scattered ray, it is only necessary to introduce the factor $\cos \alpha$ in order to make the preceding expression applicable.

* The factor π was inadvertently omitted in the original memoir.

If the direction of the original vibration be inclined at an angle θ to O Z, and that of the scattered ray at an angle ϕ , we have on resolution

$$\tan \phi = \cos \alpha \tan \theta,$$

which expresses the important law enunciated by Stokes*.

The preceding investigation depends upon the smallness of $D' - D$ as well as of A ; but where the original vibrations are parallel to the bar, the result is correct to all powers of $D' - D$. I find that, if the bar be circular and composed of material for which the density is D' and rigidity n' (the corresponding quantities for the rest of the medium being D and n), the expression for the scattered vibration is

$$\gamma = \frac{2\pi A}{\lambda^{\frac{3}{2}} R^{\frac{1}{2}}} \left\{ \frac{D' - D}{2D} - \frac{n' - n}{n' + n} \cos \alpha \right\} \times \cos \frac{2\pi}{\lambda} \left(bt - R - \frac{\lambda}{8} \right),$$

corresponding to

$$\gamma = \cos \frac{2\pi}{\lambda} bt$$

for the incident light at the centre of the bar. If we suppose that $n' = n$, this agrees with the result already found; and it is correct if the bar be small enough, whatever may be the magnitude of $\frac{D' - D}{D}$.

XXVI. On some Phenomena of Polarization by Diffusion of Light.

By J. L. SORÉT†.

IN a recent memoir of great interest‡, M. G.-A. Hirn has occupied himself with the optical properties of flames; and, for the explanation of the whole of the phenomena, he suggests the hypothesis that the solid incandescent particles to which the flame owes its brightness become transparent at that high temperature and have no longer any sensible reflecting-power. A section of the memoir is devoted to the phenomena of polarization in, or rather to their absence from, the light of flames, and contains the account of some experiments on the effect obtained by causing the light of the sun to fall on a flame and on the smoke which may escape from it§.

* "Dynamical Theory of Diffraction," Camb. Trans. vol. ix.

† Translated from the *Bibliothèque Universelle, Archives des Sciences* for November 1873, vol. xlviii. pp. 231-241.

‡ "Mémoire sur les propriétés optiques de la flamme des corps en combustion et sur la température du Soleil," *Annales de Chimie et de Physique*, November 1873.

§ We will quote the following passage:—

"One of Arago's first observations in optics demonstrated that the light

This publication of M. Hirn's induces me to make known a few of the results I have obtained in observing the polarization

of flame in general presents no trace of polarization. As is known, it was this observation that served to place one of the first and most important landmarks of the theory of the sun, by informing us that the solar light emanates from a gas, and not from a liquid or a solid.

"When we have to do with a homogeneous flame, such as that of strongly compressed hydrogen burning in compressed oxygen, when the light emanates from all parts of the incandescent gas itself, the fact discovered by Arago exhibits nothing unusual, nothing inconsistent with other known facts. It is not so when the flame is *heterogeneous*, formed of a real mixture of a gas with the dust of a solid body—dust which is far from being, as has often been erroneously advanced, in a state of infinitesimal division; in a word, it is not so with nine tenths of ordinary flames, the brightness of which can only be explained by Davy's theory. Here, in fact, not only does each of the solid particles emit light of its own, but it *must reflect* light from other sources; for it is illumined by the other particles. Then how is the total absence of polarization to be explained? The interpretation, it seems, is not difficult in the case of carburetted gases.

"Of all known substances, carbon (at least in its most usual state) is that which reflects light the least. Lampblack, for example, reflects very little or none at all; now, in flame, it is precisely in the form of lampblack that carbon is found, although incandescent. The absence of light polarized by reflection seems therefore very natural in this particular case. In an experiment, however, of which I shall speak subsequently, I have ascertained that the smoke of carbon, when produced in a very hot atmosphere and strongly illuminated, appears not black, but of dazzling whiteness. The above interpretation is therefore not so correct as it seems; and we shall see besides that it cannot be applied in its entirety in a great number of other cases. I cite as striking examples the two following:—

"1. I have examined with the Arago-lune polariscope the flame of phosphorus burning in shade or in bright sunshine, and I have not been able to perceive the least appearance of coloration in either of the two images. When, on the contrary, I directed the instrument towards the vapour of phosphoric acid strongly illuminated by the sun, the coloration became manifest.

"2. I have examined in the same manner the lofty and bright flame that issued from the top of a cupola coke-furnace, supplied with air by an energetic blast, which served for melting cast iron. Neither at night, nor in bright sunshine have I been able to perceive in it the slightest trace of polarization. Now, although here also the combustible was carbon, the light of the flame (a very bright light yellow) was certainly not due solely to particles of carbon precipitated in the burning gas; instead of *not solely*, it would be perhaps more correct to say *not at all*. When, at the end of the operation, the furnace-door was opened for drawing out the scoriæ, the calcareous flux, &c., a thick bluish smoke (various metallic oxides or salts) escaped from the top instead of the flame. This smoke, strongly illuminated by the still-glowing interior, gave to the polarimeter two images as much coloured as those obtained on looking at any light whatever, reflected under the most favourable angle, through an unsilvered glass plate. It is evidently to this vapour of metallic oxides or salts that the flame owed the greater part or even the totality of its brightness while the furnace was in action.

"How is it to be accounted for that phosphoric acid and the vapour of

of diffused light, whether by particles disseminated in a transparent medium, or by surfaces not polished. This subject, the study of which I am far from having finished, is connected with my preceding researches on the illumination of transparent bodies*.

I hardly need to recall here the fact, now well known, that a gas or a liquid holding heterogeneous particles in suspension is illuminated by the passage of natural light, and the light emanating from this illuminated trace is polarized in the plane of vision†. If the angle of vision is 90° , polarization is complete if the particles are minute and not too abundant.

This said, I will report the facts I have observed on employing lampblack as the diffuser, both at the moment of its formation and as a deposit on other bodies.

If in a dark chamber a pencil of solar rays be made to fall on a smoky flame of illuminating-gas, its trace is perceived very distinctly: the portion of the flame traversed by the pencil appears bluish white, contrasting with the reddish tint of the adjacent parts. This experiment can be easily made with a circular (Bengel) burner without a chimney; in these conditions (*i. e.* when the combustion is incomplete) the trace of the pencil of solar rays, especially if concentrated by a lens, is very visible in every part of the flame except quite at the base.

If it be observed with a Nicol or any other analyzer, for an angle of vision of 90° the polarization is ascertained to be complete, the white trace disappearing when the analyzer is turned so as to intercept the rays which are polarized in the plane of vision.

If instead of natural light a pencil of polarized solar light is employed, in the path of which a plate of quartz is interposed, the trace presents well-marked differences of coloration according to the position of the quartz.

The same phenomena are manifested when the pencil of solar light passes to the smoke above the flame—that is, to the non-incandescent lampblack.

salts or oxides, which gave to the two preceding flames their brightness, exhibited no sensible polarization as long as their particles were in the flame, while polarization became manifest outside or in the absence of the flame, although all the conditions except the temperature were absolutely the same? Are we not authorized, or even compelled to conclude that an excessively elevated temperature deprives these particles of their reflecting-power?"

* See *Archives des Sciences*, 1870, vol. xxxvii. p. 129, and vol. xxxix. p. 352.

† I name, as previously, the *plane of vision* the plane containing the incident pencil of light and the direction in which we view the illuminated body. The *angle of vision* is the angle made by the visual ray with the pencil of light on the side from which the latter arrives.

If we employ the Benger burner with its glass chimney, the flame becomes more brilliant, it ceases to be smoky, and the trace of the luminous pencil is no longer perceived; but if an excess of gas be admitted, so as to cause the flame to *shoot*, it presents in its upper part the same characters as when the burner was without its chimney.

With a butterfly burner, where the combustion is complete, no luminous trace is to be observed, and consequently no polarization.

If we examine with a polariscope a gas-flame, or better that of an oil-lamp, which shoots, without the addition of solar light, we find that the column of smoke emits light polarized in a horizontal plane.

In short, for what concerns the experimental results, it is seen that I am in accord with M. Hirn: where the flame burns with vivid brilliance the trace of a luminous pencil is not apparent.

M. Hirn (with some reserve, it is true) explains this by the weakening of the reflecting-power, and by the transparence of the carbon particles at a high temperature. I should not venture to express an absolute opinion on this question, which I have not specially studied; but before admitting this hypothesis, which is partly in disagreement with the principle of the identity of the emissive and absorbing-powers, I ask myself if the absence of polarization in the parts of the flame where combustion is complete cannot be accounted for by the two following considerations:—

1st. A gas-flame may in general be regarded as formed of several layers. In the centre of the sheet the gas is at a temperature inferior to that of the decomposition of the carburets of hydrogen; it is only on the surfaces of this sheet that decomposition is effected and carbon precipitated in incandescent particles which are burned as rapidly as they are produced. The incandescent layer is therefore very thin, the particles are rare, and the phenomena of reflection and polarization must be insensible.

The fact of the apparent continuity of the flame cannot be alleged as a proof of the abundance of the particles; irradiation and the persistence of the impressions on the retina sufficiently account for it. Besides, in speaking of the rarity of the particles I only mean a relative rarity. Observing with a lens the feeble luminous trace produced by a solar ray in very clear water suffices for the recognition of the presence of myriads of heterogeneous particles floating in the liquid; and yet the power of illumination is very feeble.

If, on the contrary, the flame is smoky, the afflux of oxygen

being insufficient, combustion does not destroy the lampblack in proportion to its production, the particles accumulate and become sufficiently abundant to render visible the trace of the pencil of solar light.

2nd. When the combustion is complete the light is very intense, it dazzles the eye; and it is easy to comprehend how difficult it must be to perceive the polarization of the very slight proportion of reflected light. We can convince ourselves of this by endeavouring to observe through a gas-flame the polarization which accompanies the illumination of a liquid for example, though it is so easily verified under the usual conditions.

I have also studied the diffusion of light by any solid surface covered with lampblack deposited from the flame of essence of turpentine or of benzine. When a pencil of sunlight falls on this surface in a dark chamber, a very notable proportion of light is seen to be diffused in all directions; the incident pencil is marked on the black by a white spot of considerable splendour, much more brilliant than that which is obtained upon a smoky flame: the reflecting particles are, in fact, much more abundant. Whatever be the angle and the plane of incidence of the luminous pencil relative to the blackened surface, the diffused light is seen to be polarized in the plane of vision. The maximum corresponds to a visual angle of 90° , though there the polarization is not complete. When looked at through an analyzer which is rotated, the spot augments or diminishes in brightness, but remains always distinctly visible.

If in place of natural light we employ a pencil of solar rays polarized, for instance, in a vertical plane, the light diffused by the lampblack presents in all directions the same phenomena of partial polarization as the trace which is produced in an illuminated gas or liquid. In a horizontal plane of vision, and with a visual angle of 90° , we have the minimum of brightness, but not complete extinction as this takes place with a smoky flame or, in general, with gases or liquids holding in suspension fine particles in relatively little abundance. The residual light is neutral and shows no trace of polarization; but as soon as the angle of vision becomes greater or less than 50° partial polarization reappears immediately.

I explain this residue of neutral light by multiple reflections from the minute rugosities produced by the particles of lampblack on the diffusing surface. I do not think it can be attributed to a fluorescence, for the following reasons:—

1st. When this residual light is analyzed with the spectroscope, the principal lines of the solar spectrum are recognized in it; therefore there is no change of refrangibility*.

* This proof is perhaps not quite conclusive; for under the influence of *Phil. Mag. S. 4. Vol. 47. No. 311. March 1874.* P

2nd. M. Ed. Becquerel has proved that lampblack deposited from the flame of benzine upon a plate of mica, in a layer sufficiently thin to be translucent, does not give in the phosphoroscope any trace of phosphorescence*. Now in general, with solid substances, fluorescence has a certain duration and is confounded with phosphorescence.

3rd. If this residual light proceeded from a fluorescence, it ought to be remarked also in the smoke of a flame; the intensity of the illumination is certainly strong enough for this. Now, as we have said, polarization is complete in this latter case. One can very well conceive that, the particles being relatively much disseminated and distributed throughout the thickness of the incident pencil, the proportion of light undergoing multiple reflections may be insensible, while in the lampblack deposited on a solid substance all the particles are in mutual contact, they are accumulated on the same surface, and must reflect reciprocally a notable quantity of light.

In brief, lampblack, whether as a continuous deposit or in the state of corpuscles disseminated in a gas, appears to me to constitute a very good type for this kind of phenomena of illumination. Indeed, on the one hand these particles are very tenuous, and, on the other, they are opaque—which eliminates the perturbations which may be induced by refraction, leaving only what is due to reflection.

There are other fumes, in the widest sense of the term, which give rise to very different results. For example, the vapour of water precipitated in small quantity in the vesicular state, and illuminated by a pencil of light, gives also phenomena of polarization; but while ordinary vapour reflects under an angle of vision of 90° light polarized in the plane of vision, vesicular vapour in the same conditions diffuses light partially polarized in a perpendicular plane. This is what Professor Tyndall has described in his beautiful researches on the optical properties of cloudy substances†, and I have verified also upon vapour condensed in divers manners.

For an angle of vision less than a right angle a diminution of the polarization is first observed, then a neutral point (towards 70°); at last, under an angle of about 65° and below, polariza-

the solar rays certain substances, as sulphur and carbon, emit a light which has the closest correspondence with that of the fluorescence of quinine, esculine, &c., except that there is no change of refrangibility: it is what M. Lallemand has called isochromatic fluorescence.

* M. Ed. Becquerel has been so extremely obliging as, at my request, to repeat this experiment in my presence.

† See *Archives des Sciences*, 1869, vol. xxxiv. p. 172.

tion reappears in the contrary direction; that is, the light is polarized in the plane of vision.

The fumes obtained on agitating some drops of nitric or chlorhydric acid in a moistened balloon, or chlorhydric acid and ammonia, give also anomalous polarization for an angle of vision of 90° .

On the contrary, on clouds and fogs illuminated by the sun I have never observed polarization in a plane perpendicular to the plane of vision; the diffused light is either almost neutral or polarized normally. With a light mist the polarization is very marked. The white bow is strongly polarized in the plane of vision.

When water is pulverized by making a very thin jet strike forcibly against a solid body, a vapour is obtained composed of minute drops. The light diffused by it is partially polarized in the plane of vision for a visual angle of 90° . Towards 100° a neutral point is observed*; for greater angles the polarization is inverse, by refraction. If, starting from 90° , the angle of vision is diminished, the polarization remains normal; it is very strong in a direction corresponding approximately to the position of the first two rainbows. I say approximately, because the two rainbows, which are usually so readily visible, disappear almost completely in these conditions of extreme minuteness of the globules of water; they only manifest themselves by two maxima of light sensibly white, between which a red coloration is observed. The other colours cannot be distinguished; it seems that they have spread out and mingled, and that the reds of the two bows have united with each other. This remarkable fact is, I think, thus explained:—because on extremely minute surfaces reflection and refraction do not follow the laws of geometrical optics; as Fresnel has demonstrated, diffraction is produced.

I will not add more; the examples just cited suffice to show the considerable influence exercised by the state in which the particles are found. I defer the publication of the other facts which I have hitherto been able to observe, or which I may ascertain in pursuing this study on diffusion.

* The position of neutral points in general is not very constant; it varies, as Tyndall has shown, with the circumstances of magnitude and abundance of the particles.

XXVII. *On the Transformation of the Vibroscope into a Tonometer, and its employment for the Determination of the Absolute Number of Vibrations.* By A. TERQUEM*.

By the optical study of vibrations and the construction of the vibroscope, M. Lissajous has endowed acoustics with means of investigation much more accurate than those based upon hearing only; nevertheless this optical method does not appear hitherto to have been conveniently applied to the determination of the absolute number of vibrations. I have thought that it could be utilized for this purpose, and that a tonometer might be more easily constructed with its aid than by the operation devised by Scheibler. The new tonometer, at least as accurate, would be much less [costly; and it could be employed to give, by simple reading, the number of vibrations of any sonorous body whatever, and that through a very great extent of the musical scale.

I have had made by M. König four diapasons furnished with cursors, and each carrying at the extremity of one of its branches, like the diapason of the vibroscope, a small biconvex lens to serve as an objective. These diapasons can be successively fixed upon the same support, on which an ocular is placed. By shifting the cursors, we can obtain all the sounds comprised between UT_2 (128 double vibrations) and UT_3 ; moreover some sounds are common to two diapasons which immediately follow one another. The diapasons were divided by M. König after his tonometer, in such sort that, by shifting the cursors the space which separates two strokes, the sound is changed two double vibrations; but I consider this only a completely arbitrary division which might be replaced by any other made by means of a dividing machine. I shall give to these diapasons the name of *standards*.

I have, besides, other diapasons, not graduated, but likewise furnished with cursors and extending from UT_2 to UT_3 . Two are sufficient, because spare cursors can be adapted to them. I shall give to these the name of *auxiliary* diapasons.

The first standard diapason and the first auxiliary are fixed the one in front of the other, on suitable supports, in one and the same horizontal plane, and at a right angle; moreover the vibrations of the standard diapason (which serves as a vibroscope) being made as usual, in a vertical plane, those of the auxiliary take place in a horizontal plane.

That the curves resulting from the coexistence of the two vibratory movements at a right angle might be readily perceived, particles of finely powered antimony have been fixed with gum

* Translated from a separate impression, communicated by the Author, from the *Comptes Rendus de l'Académie des Sciences*, January 12, 1874.

upon the terminal section of one of the branches of the auxiliary diapason. The facets of this crystalline powder, obliquely illuminated by means of a lamp and lenses, form luminous points of great brilliancy and extreme delicacy. The cursor of the standard having been fixed upon the first stroke of the division, the auxiliary diapason is put into exact unison with the former by shifting the cursors, finally putting on them small pieces of wax. The operator is guided in this operation evidently by the transformations of the elliptic curve which is due to the coexistence of the vibrations; and he stops when it diminishes gradually and very slowly without changing its form.

This done, he moves the cursor of the standard diapason so as to obtain about one beat per second, and determines the exact duration of at least fifty beats by aid of a pointing telltale, observing the oscillations of the elliptic curve. If the conditions be good, the total duration of fifty beats scarcely varies a half second in several successive determinations, which gives an approximation within $\frac{1}{100}$ of a second for one beat, and consequently permits the number of vibrations to be determined within less than 0.01. The cursor of the auxiliary diapason is then shifted till unison is re-established, which is ascertained by the fixity of the curve produced. The cursor of the standard diapason is then placed on the second stroke of the division, and the duration of the beats produced determined again.

The operation is continued in the same manner by shifting successively the cursors of the two diapasons until a sound is obtained sufficiently distant from UT_2 , such as MI_2 , capable of giving with the sound UT_2 a sufficiently simple acoustic curve (4:5). When, by successive stops, the auxiliary diapason is put in unison with the sound MI_2 , the cursor of the first standard diapason must be carried back to the first stroke, and it must be ascertained if the sound MI_2 is perfectly true; if there is any difference, it is determined by the duration of the oscillations of the acoustic curve. The sum total of the numbers of beats per second (the inverse of their duration) which have been observed on the shifting of the cursors in succession, gives the difference of the numbers of vibrations of the sounds UT_2 and MI_2 , of which we have the ratio—which agrees with the principle given by Scheibler for the construction of the tonometer, and permits the absolute number of the vibrations of UT_2 to be calculated.

Continuing in the same manner, we arrive at UT_3 . On the way numerous datum- and verification-points are met with in the simple intervals, such as the fourth, the fifth, the sixth, &c. At length we know in this manner the absolute number of vibrations corresponding to each stroke of the division of the standard diapason.

The only practical difficulty which I have encountered (and this I think I am on the point of surmounting) is in the mode of fixing the cursors on the diapasons. In fact they should be fixed there in a quite invariable manner, so that the sound may not suffer any variation of pitch when the diapason is set in vibration; this does not always happen, though the differences observed are very slight. In the second place, the cursors must be restored to the same position with mathematical accuracy.

I have already verified a certain number of the divisions traced by M. König on my diapasons from his tonometer, and have found only differences amounting to not more than a few hundredths of a vibration per second, which shows the degree of confidence that may be placed in the determinations made from the tonometer constructed by him.

By this method, then, it will be possible, with more facility than by the old process, though not without sustained attention and numerous determinations, to divide the interval from νT_2 to νT_3 into sounds differing one from another by two vibrations, or even by one only, if it be desired. At the least, by the employment of auxiliary diapasons with cursors, the same procedure will permit the verification of the accuracy of the divisions of the standard diapasons mounted in the form of a vibroscope, and to construct a table of corrections if the divisions are not perfectly exact.

The vibroscope tonometer once constructed and verified, in order to determine the pitch of any sound whatever, it will be sufficient to place one of the diapasons of which it consists by the side of the vibrating body in such a way that the vibrations of the one shall be perpendicular to those of the other. On this body (which may be a rod, a plate, a string, &c.) some particles of antimony powder are to be fixed; then the cursors will be shifted until an acoustic curve of well-recognizable form is obtained, such as that due to vibrations in the ratio of 1 : 2, 1 : 3, 1 : 4, 1 : 5, ... If the sound studied is higher or deeper than those comprised in the interval from νT_2 to νT_3 , it is not even necessary to obtain the absolute fixity of the curve; in fact, by determining the duration of the period of return of the same figure, we can know the difference between the number of vibrations of the sound and that which it ought to have in order that that fixity may exist.

With this tonometer I purpose to resume, with more accuracy than has hitherto been attainable, the study of the vibrations of rigid bodies, particularly plates, in order to arrive, if possible, at elucidating the question, still so much controverted, of the propagation of sound in bodies which present at least two dimensions of the same order of magnitude.

XXVIII. *Notices respecting New Books.*

An Introduction to the Elements of Euclid, being a familiar explanation of the first twelve propositions of the first book. By the Rev. STEPHEN HAWTREY, A.M., late Assistant Master at Eton. London: Longmans, Green, and Co. 1874. (Pp. 105.)

ONE'S first impression on looking at this book might be to ask, Is it possible that the first twelve propositions of Euclid's first book require 105 pages of explanation? But if the querist has ever taught the elements of geometry to a child and remembers in what his verbal instruction consisted, he will probably find on reading the book that his explanations were at least as voluminous as those here printed; and doubtless he will be both interested and instructed in this record of the method of oral teaching which was used for many years by Mr. Hawtreys "with some success."

We will notice very briefly the leading points of the book. In the first place the author insists on the learner being provided with "a pair of compasses having a pen- and pencil-leg, a small flat ruler, and a hard pencil;" besides these, he adds, it is well to have "a little box-wood triangle, called . . . 'a set square.'" He also insists on having the *data* of each problem drawn in ink before the construction is completed in pencil. In the next place he treats the definitions very fully, those at least which the learner needs to begin upon; the postulates and axioms are noticed, with a sufficiency of comment, the twelfth axiom being left out as not needed at present. The propositions are then taken in order, directions are given for drawing the figure line by line, and each step of the demonstration commented on. At the end of each exposition the proposition is given as it stands in Euclid—general enunciation, particular enunciation, construction, demonstration; and the learner is directed to write it out.

The fourth proposition is treated at great length, and its importance in regard to what follows insisted on more forcibly (not than it deserves to be, but) than we have ever seen it insisted on before. "It is a great and most important proposition, and has been called the key to Euclid." The exposition of the proposition is followed by seven exercises, all fully worked out; and as they cannot be done without assuming something more than the first four propositions, the assumptions are carefully explained; as a deduction from two of the exercises the fifth proposition is proved in fact though not in name. After this the learner is ready to encounter the fifth proposition in its proper form, which is, as Mr. Hawtreys says, nothing but an exercise on the fourth proposition, though too hard to be taken immediately after it.

We fear that this account of the contents is somewhat meagre; but there is, in fact, not much to say about the book in the way of description. Its merit consists in the admirable clearness with which all the points of a well-trodden road are brought under notice and much insisted on, which, in these days of quick travelling, is

apt to be passed over. The book is interesting in another way; it is quaint without affectation, and produces a very distinct impression of being written by a man of a genial and original character. Here and there little pieces of classical lore are felicitously introduced, and the more appropriately as Eton was the scene of Mr. Hawtrey's labours. The dedication runs thus:—"Μίστραις suis ἀγαπητοῖς hoc opusculum dicat S. H." The "initiated" are those pupils who have mastered the fourth proposition—who can not only write it out, but understand and are able to apply it—who having got hold of the key, can go on to investigate the hidden treasures of Euclid. We cordially recommend the book to all who are engaged in teaching the elements of geometry to the young; they cannot fail to be interested, and will probably learn from it something as to the best way of performing a difficult and important task.

XXIX. *Proceedings of Learned Societies.*

ROYAL SOCIETY.

[Continued from p. 74.]

May 8, 1873.—Francis Sibson, M.D., Vice-President, in the Chair.

THE following communication was read:—

"The Action of Light on the Electrical Resistance of Selenium."
By Lieut. Sale, R.E.

It having been recently brought to notice that selenium in the crystalline condition exhibits the remarkable property of having a conductivity varying with the degree of light to which it is exposed, the following experiments were undertaken with a view to the further elucidation of the matter:—

Experiment 1.—A bar of crystalline selenium measuring approximately $1.5'' \times .5'' \times .05''$ was procured, and platinum wire terminals were fastened to the ends.

The bar itself was then enclosed in a box having a draw-lid, so as to admit or exclude the light at pleasure.

Then, the lid of the box being on, the resistance of the selenium was measured by means of a high-resistance galvanometer and a Wheatstone's bridge, with dial-coils capable of measuring up to 10,000,000 ohms. The battery-power was 2 cells Daniell.

The measurement was made on a dull cloudy day, and in a room of equable temperature.

The resistance having been carefully balanced, the lid of the box was withdrawn, when the resistance of the selenium fell instantaneously and considerably, as indicated by the rapid movement of the spot of light on the galvanometer-scale.

Experiment 2.—The transition from darkness to the light given by an ordinary gas-burner (conditions as before), caused a slight and barely perceptible fall in the resistance.

Experiment 3.—The bar of selenium was next tried in the solar

spectrum on a very bright cloudless day (conditions as before), except that more battery-power was used (10 cells Daniell).

The diffused daylight could not be cut off; so the trial was made in the most shaded part of an ordinary room, the spectrum being superimposed on the ordinary diffused daylight.

The resistances were very carefully balanced in each case, with the following results:—

Resistance in darkness	330,000
" " violet	279,000
" " red	255,700
" " orange	277,000
" " green	278,000
" " blue and indigo	279,000
" " centre of red	255,000
Resistance just on the outside edge of red, red side	220,000
Resistance in dark rays clear of red	228,000
Resistance in diffused daylight only	270,000
Resistance taken in the dark immediately after exposure (resistance rising)	310,000

The indications were very clear; and the bar of selenium was so sensitive to the action of the spectrum, that a slight movement of the prism produced a corresponding movement in the spot of light on the galvanometer-scale.

It is to be noted that in this experiment the reflecting galvanometer was placed on a heavy masonry pillar insulated from the floor for observatory purposes, and that the battery, bar of selenium, and resistance-coils were in another room, being connected by long and carefully insulated leads with the galvanometer.

Experiment 4.—The diffused light was cut off as much as possible by screens; and the resistances were again balanced in the solar spectrum. Conditions as in the last case.

Resistance of selenium in red	240,000
" just outside red	240,700
" in blue	270,000
" in such diffused light as came through screens	290,000
Light cut off by lid of box (resistance rising)	310,000

Experiment 5.—The selenium was also exposed to the spectrum of the electric light in a darkened room.

The effect was feeble; but by using more battery-power in balancing, it was possible to measure the swing of the spot of light when the selenium was suddenly exposed to the action of the light of the spectrum.

The maximum effect was obtained in, or just at the edge of the red, the violet and blue rays producing scarcely any effect.

Experiment 6.—The selenium was exposed to the full sunlight; the resistance fell enormously and instantaneously, and on balan-

cing it was found to be little more than half what it was in the darkness.

The following were the general results of the experiments :—

Results.—(1) That the resistance of selenium is largely affected by exposure to light.

(2) That this effect is not produced by the actinic rays, but is at a maximum at, or just outside the red rays, at a place nearly coincident with the locus of the maximum of the heat-rays.

(3) That the effect of varying resistances is certainly not due to any change of temperature in the bar of selenium.

(4) That the effect produced on exposure to light is sensibly instantaneous, but that, on cutting off the light, the return to the normal resistance is not so rapid.

It would seem that there exists a power in rays nearly coincident with the heat-rays of high intensity, of altering instantaneously and without change of temperature the molecular condition of this particular element.

May 15.—William Spottiswoode, M.A., Treasurer and Vice-President, in the Chair.

The following communications were read :—

“Determination of the Number of Electrostatic Units in the Electromagnetic Unit made in the Physical Laboratory of Glasgow University.” By Dugald M’Kichan, M.A.

The object of this paper is to describe experiments made at intervals from 1870 to 1872 in the Physical Laboratory of Glasgow University to determine the relation between the fundamental units in the two systems of absolute electrical measurement, the electromagnetic and the electrostatic. A summary is also given of the results of similar observations made by W. F. King in 1867 and 1868.

The two systems of electrical measurement, or the units which they employ, are founded on the fundamental units of time, mass, and space applied to the observed effects of electricity at rest and electricity in motion. The dimensions of quantity in the two systems are such that the ratio of the electromagnetic and the electrostatic unit of quantity is expressible as a velocity.

This velocity, usually known as v , is not only of great importance in all combinations of electromagnetic and electrostatic action, but it is also of great scientific importance in the theory of the propagation of electromagnetic disturbances through a dielectric medium. It occupies a very important place in the development of the electromagnetic theory of light by Professor Clerk Maxwell, according to whose theory this velocity v is the same as the velocity of light.

The first experimental determination of v was made by Weber from a common electrostatic and electromagnetic measure of capacity. As the result of Weber’s experiments, v was found to be 310.74×10^8 centims. per second.

Another determination was made by Prof. Clerk Maxwell in

1868, by means of a direct comparison of electrostatic attraction with electromagnetic repulsion. His experiments gave $v = 288 \times 10^8$ centims. per second.

The value of v given by the experiments here described is 293×10^8 centims. per second. The method employed was that of obtaining an absolute electrostatic and an absolute electromagnetic measurement of the same electromotive force. v is defined as the ratio of the units of quantity in the two systems; but it follows from the definition of electromotive force, that v is also the ratio of the units of electromotive force in the two systems.

The electromotive force, or the difference of potentials between the two poles of a constant Daniell's battery, was measured electrostatically by means of Sir William Thomson's absolute electrometer. The absolute electromagnetic value of this electromotive force was given by the effect of the current which it maintained in the circuit of an electro-dynamometer. The determination of this value depended on the resistance of the electro-dynamometer-circuit, which was reckoned in terms of the absolute value of the British-Association standard unit of resistance. Any correction which may hereafter be found to be applicable to the absolute value of this standard coil, as measured at King's College by Professors Clerk Maxwell, Balfour Stewart, and Fleeming Jenkin, must be applied to the value of v given above.

The comparisons made in 1867 and 1868 by Mr. King gave as the mean value of v , 284.6×10^8 centims. per second.

The experiments made in 1870 with the new absolute electrometer gave as the mean result $v = 294.5 \times 10^8$ centims. per second. The result of later observations made under much more favourable circumstances was $v = 292.4 \times 10^8$ centims. per second.

The latest observations (1872) furnish the most probable value of v , 293×10^8 centims. per second.

"On the Heating of a Disk by rapid Rotation in vacuo." By Balfour Stewart, M.A., F.R.S., and P. G. Tait, M.A.

26. In two previous communications (Proc. Roy. Soc. June 15, 1865, and No. 88, 1866) to this Society, we gave an account of some experiments which we had made upon the heating of a disk through rotation in vacuo. In these experiments the increase of radiation of the heated disk was observed by means of a delicate thermopile and galvanometer. Three aluminium disks of various thicknesses and one ebonite disk were used; and the results derived from the experiments were as follows:—

(1) The heating effect observed appeared to be independent of the density, and of the chemical constitution, of the residual air and vapour surrounding the disks.

(2) The quantity of heat developed under similar circumstances of rotation in three aluminium disks .05, .0375, .025 of an inch in thickness respectively appeared to be the same, inasmuch as the relative thermometric effect for these disks varied inversely as their thickness.

(3) Besides the heating effect alluded to in (1) and (2), there was found to be, when the vacuum had been recently made, a strictly temporary effect, sometimes in the direction of heat, sometimes in that of cold, owing probably to the condensation or evaporation of small quantities of aqueous vapour; but this effect was only noticeable during rotation, disappearing the moment the motion was stopped.

27. The experiments described in these communications were resumed in 1870. In the interval an addition had been made to the apparatus, in virtue of which an ordinary carbonic-acid vacuum might be subjected to the influence of a vessel containing potash allowed to open in it, and thus to absorb as much as possible of the remaining gas.

On May 4 a carbonic-acid vacuum was obtained by this means (pressure 0.05 in.). A disk made of cartridge-paper, when made to rotate in this vacuum, gave a very perceptible result. Carbonic acid was then allowed to enter the vacuum until the pressure became 0.65 in. The consequence of this increased pressure was in this instance an increase in the effect, which was probably of a permanent nature, inasmuch as it remained after three days. Unfortunately the exact increase was not noted; but it is believed that the heat-indication became about three times as great in consequence of the additional pressure.

28. On May 11 another carbonic-acid vacuum was made (pressure 0.12 in.), and at the suggestion of Professor Maxwell a sulphuric-acid gauge was placed in the receiver. A rotation was then made; and the result of the rotation was a hardly perceptible rise in the sulphuric-acid gauge. We may therefore imagine that the residual air was not greatly heated.

29. On May 16 another carbonic-acid vacuum was obtained (pressure 0.08 in.), and with an ebonite disk of about $\frac{1}{20}$ inch thickness, the heat-indication was 16. Carbonic acid was introduced until the pressure was 1.6 in.; but the indication was only 18. This result is in conformity with our previous experiments, in which an increase of pressure of the residual air produced little or no effect.

30. The ebonite disk was likewise tried in a carbonic-acid vacuum, pressure 0.04 in.; and also in one, pressure 0.02 in., which was the lowest we could obtain. The result of these experiments appeared to show that in all probability the cartridge-paper disk radiated more than twice as much as the thin ebonite disk. The experiments were put a stop to by a collapse of the glass receiver during rotation, fortunately without injuring any one present.

31. In June 1871 the experiments were resumed. In the mean time Mr. Beckley had fitted the apparatus with an arrangement working through a barometer-tube, by means of which, instead of trusting to radiation, the disk itself might, after rotation, be tapped by means of the pile, which could be brought up to it and then withdrawn. By this means a much larger effect might be obtained; and it became possible, by varying the adjustment, to find according to what law the heat effect varies with the distance from the centre.

32. These experiments were conducted in the following manner:—The disk was first of all tapped before rotation several times; at each tapping the momentary swing of the needle was recorded, and the mean of the readings was regarded as indicating the state of the disk with respect to heat.

The disk was next tapped after rotation, and the difference between the readings before and after was taken as indicating the change in the state of the disk produced by rotation. In the later experiments the disk before rotation was kept in slight motion in order to equalize any tendency to unequal heating of its various parts; but this was not done in the experiments of June 1871. In these experiments (June 1871) the disk was of brown paper, and the results obtained were as follows:—For a carbonic-acid vacuum (pressure 0·065 in.) a swing of 307 divisions was recorded, while for a hydrogen vacuum (pressure 0·150 in.) a swing of 281 divisions was recorded. Each result was the mean of three rotations.

33. The next experiments were made in January 1872. The galvanometer was one of Thomson's, but more adapted for battery currents, and hence not in a very delicate state for these experiments; the time of vibration of the needle was 3 seconds. The disk used was of ebonite (thickness about $\frac{1}{16}$ in.). The ebonite of this disk was completely black; and in this respect, as well as in being thinner, it differed from the ebonite disk first used in the radiation-experiments (art. 17). In the present experiments the centre of the pile was made to tap the disk at a distance of 1·5 in. from the rim. The amount and velocity of rotation were represented by 30 turns of the handle, or 3750 turns of the disk, in about 40 seconds. The following results were obtained, a result representing on an average somewhat more than four rotations:—

	Pressure of residual gas, in inches.	Heat-indication for		
		Dry hydrogen.	Dry air.	Dry carbonic acid.
(A)	$\frac{9}{20}$	11·5	35·0	33·0
(B)	$\frac{3}{20}$	7·5	15·0	15·0

It would thus appear that the results derived by tapping are very different from the radiation-results, inasmuch as in the former the effect of the pressure and quality of the residual air is very apparent, while in the radiation-results it is hardly perceptible. A probable explanation of this will be given afterwards (art. 46); but in the mean time, in view of these results, it will be expedient to discuss them quite independently and by themselves, with the view of ascertaining whether they can best be explained by a gas-effect alone, or whether they likewise indicate a residual effect independent of gas.

34. With this object let us take $\frac{(A)+(B)}{2}$ as representing the whole effect at a pressure of $\frac{6}{20}$ in., due to whatever cause or causes. We thus obtain

	Dry hydrogen.	Dry air.	Dry carbonic acid.
Whole effect at $\frac{6}{20}$..	9·5	25·0	24·0

Again, let us suppose that (A) — (B) denotes the *gas-effect* for $\frac{6}{20}$ in., and we obtain

	Dry hydrogen.	Dry air.	Dry carbonic acid.
Gas-effect at $\frac{6}{20}$	4.0	20.0	18.0

Finally, let us regard as *unknown residual effect* the difference between the *whole effect* and the *gas-effect*, and we obtain

	Dry hydrogen.	Dry air.	Dry carbonic acid.
Residual effect	5.5	5.0	6.0

35. Similar experiments with the same galvanometer were made with a disk of cartridge-paper, of which the pores were filled with solid paraffin. And here we may mention that in all experiments with paper disks a small wooden attachment was placed at some little distance behind the disk and at the height of the pile, and against this the disk was pressed during tapping—care being taken that the disk did not touch it during motion, but only when it was pressed against it by bringing up the pile.

With this paper disk the amount and speed of motion were represented by 30 turns of the handle, or 3750 turns of the disk, in 30 seconds. The results were as follows, a result representing on an average somewhat less than four rotations :—

	Pressure of residual gas, in inches.	Heat-indication for		
		Dry hydrogen.	Dry air.	Dry carbonic acid.
(A)	$\frac{9}{20}$	27.0	55.0	55.0
(B)	$\frac{3}{20}$	23.0	35.0	32.0

Treating these results in the same manner as those of the ebonite disk, we obtain :—

	Dry hydrogen.	Dry air.	Dry carbonic acid.
Whole effect ($\frac{6}{20}$) . .	25.0	45.0	43.5
Gas-effect ($\frac{6}{20}$)	4.0	20.0	23.0
Residual effect	21.0	25.0	20.5

36. Now, if we suppose that there is only one effect due to gas, it follows :—

(α) That the proportion between the effects due to the various gases experimented on (and all of the same pressure) is nevertheless different for the two disks.

(β) That the proportion (for the same disk) between the effects due to the various gases experimented on is different according to the pressure.

If, however, we suppose that there are two effects, one of which is independent of the residual gas, we find :—

(α) That, as regards the *gas-effect*, the proportion between that due to the various gases is nearly the same for both disks. Thus in the ebonite disk we have 4, 20, 18, while in the paper disk we have 4, 20, 23 as representing the gas-effect for the various gases.

(β) That the *residual effect* in either disk is nearly the same for the various gases. Thus in the ebonite disk we have 5.5, 5.0, 6.0, while in the paper disk we have 21.0, 25.0, 20.5 as representing the residual effect for the various gases.

The results are thus much more simple on the hypothesis of two effects, one of these being independent of the residual gas, than on the hypothesis of only one effect.

37. It was next endeavoured to ascertain whether these two effects were differently influenced by a blind.

A linen blind was first used before the thin ebonite disk, which it cloaked during rotation, falling down afterwards. During rotation the blind did not appear to touch the disk. A delicate galvanometer was used for this experiment; and the time of vibration of the needle was 2 seconds. The rotation consisted of 30 turns of the handle in 40 seconds, as before. The following result was obtained (each number representing the mean of two observations):—

	Without blind.	With blind.
$\frac{3}{20}$ of dry hydrogen	165	127
$\frac{3}{20}$ hydrogen + $\frac{6}{20}$ air	477	248

Thus the proportion between the two effects is greatly altered by the blind; for while the hydrogen effect is not much diminished, the other is reduced to little more than one half.

38. A chamois-leather blind was next used, which lay close to the disk during rotation. For all these experiments the time of vibration of the galvanometer-needle was 20 to 30 seconds, while the speed was the same as before. Also the disk was tapped immediately after rotation, and then again at the interval of one minute; and in order to obtain as many individual results as possible, the mean of these two tappings has been taken in the following comparison:—

	Without blind.	With blind.
$\frac{3}{20}$ of dry hydrogen .	53 { mean of 76 individual observations.	52 { mean of 22 individual observations.
$\frac{3}{20}$ hydrogen + $\frac{6}{20}$ air	217 { mean of 68 individual observations.	140 { mean of 16 individual observations.

Here also we find that the proportion between the two effects is greatly altered by the blind; so that while the hydrogen effect is not much stopped, the other is diminished very considerably.

39. A set of experiments of 8 observations, each similar to those of arts. 33, 34, 35, were made with the blind on for the two gases, dry hydrogen and dry air. From these we obtain the following:—

	Pressure of residual gas, in inches.	Heat-indication for	
		Dry hydrogen.	Dry air.
Blind on	$\frac{9}{20}$	83	178
	$\frac{3}{20}$	52	82

From which we deduce:—

Residual effect, blind on	36.5	34
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Again, from certain experiments with the blind off (each result

being the mean of 6 observations), we obtain the following effect :—

$$\text{For dry hydrogen} \dots \left\{ \begin{array}{ll} (\frac{9}{20}) & 88 \\ (\frac{3}{20}) & 50 \end{array} \right.$$

and hence we find residual effect = 31.

From all these results, allowing for errors of experiment, we may suppose that the residual effect is not much altered by a chamois-leather blind.

We ought to mention that the numbers of this article, similarly to those of art. 38, represent the mean of two tapplings, the one taken immediately after rotation, and the other after the interval of one minute.

40. It was suggested to us by Prof. Helmholtz that it would be desirable to ascertain whether any difference was produced in the results by loading the disk on one side; for if these results were due to vibration, it might be supposed that they would be affected by this means.

It will be seen by art. 39 that the residual effect obtained from a disk covered with chamois leather is approximately the same as that from an uncovered disk; this would appear to us to be against the vibration hypothesis.

In the following experiment the disk was covered with a chamois-leather blind with a segment cut out as underneath—



A mean of two sets of experiments (consisting of 16 separate observations, and embracing results immediately after rotation, as well as results one minute after) gave as the result of tapping in A with $\frac{3}{20}$ dry hydrogen 47, while a mean of 24 similar observations with the same atmosphere gave as the result of tapping an uncovered disk 53. Allowing for errors of experiment, we may conclude that this arrangement does not much influence the results.

41. The disk was next treated in the following manner :—



It was covered with a chamois leather blind tied into holes drilled in the disk, and having the above shape. The mean of 3 sets of experiments similar to those in art. 40, and consisting of 18 separate observations, gave as follows for an atmosphere of $\frac{3}{20}$ dry hydrogen, tapping in A 54, tapping in B 56, which two results are approximately equal to one another, the mean being 55.

The mean of 3 sets of experiments, consisting of 24 separate observations for an atmosphere of $\frac{3}{20}$ hydrogen, but with an uncovered disk, gave 53 as the heat-result. All these experiments apparently combine to prove that the result is not due to vibration.

42. When the atmosphere was $\frac{3}{20}$ hyd. + $\frac{6}{20}$ air, we also found A and B approximately the same, A giving 181 while B gave 182; we have therefore not hesitated to combine together the results of tapping in this atmosphere, whether at A, or at B, or with an uncovered disk, in the comparison given in art. 38.

43. Our next experiments were made with the view of testing whether or not the two effects, the residual and the gas-effect, were resident in the same particles of the disk; and for this purpose the experiments made immediately after rotation were compared with those made one minute afterwards.

The experiments available for this purpose are so numerous that they can bear splitting into two portions, in each of which the same result is seen.

Thus we have for an atmosphere of $\frac{3}{20}$ dry hydrogen, and as the mean of 30 individual comparisons,

Effect at first : Effect one minute after :: 1.30 : 1;

also, as the mean of 22 individual comparisons, we obtain the proportion of 1.19 : 1, while as the mean of the whole we obtain 1.25 : 1.

Treating in a similar manner the observations made with an atmosphere of $\frac{3}{20}$ hyd. + $\frac{6}{20}$ air, we obtain

As the mean of 25 comparisons 1.47 : 1,

As the mean of 21 comparisons 1.41 : 1,

while as the mean of the whole we obtain 1.44 : 1.

We therefore conclude that the residual effect is less diminished during the interval of one minute than the gas-effect.

44. We next made experiments with two aluminium disks .05 and .025 of an inch in thickness respectively. These disks were covered on both sides with a coating of lampblack applied by negative photographic varnish.

In both disks we found that with a vacuum of $\frac{3}{20}$ hyd. the effect one minute after rotation was greater than the effect immediately after rotation. Thus we have:—

For thick disk (mean of 4 observations)	}	Effect at first : Effect one minute after :: .85 : 1;
For thin disk (mean of 2 experiments, embracing 7 obser- vations)		
		do. : do. :: .86 : 1.

But in cases where the gas-effect was considerable, this peculiarity disappeared. Thus we have for an atmosphere of $\frac{3}{20}$ hyd. + $\frac{6}{20}$ air :—

For thick disk (mean of 2 observations) }	Effect at first :	Effect one minute after ::	1.05 : 1.
For thin disk (mean of 2 observations) }	do. :	do.	:: 1.45 : 1.

Also, for an air-vacuum of $\frac{9}{20}$, we have

For thin disk (mean of 3 observations) }	Effect at first :	Effect one minute after ::	1.49 : 1.
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In all the experiments with aluminium disks the needle of the galvanometer vibrated 20 times in 20 seconds, and the rotation consisted of 30 turns of the handle in 40 seconds.

Without pretending to explain all these experiments*, it must, we think, be concluded from them that there are two effects which are differently distributed over the particles of the disk.

45. The relation between the effect for $\frac{3}{20}$ hyd. and that for $\frac{3}{20}$ hyd. + $\frac{6}{20}$ air for the thin aluminium disk (galv. 20 in 20 seconds) is given as follows :—

Effect for $\frac{3}{20}$ hyd. (mean of 22 observations) }	:	{ Effect for $\frac{3}{20}$ hyd. + $\frac{6}{20}$ air (mean of 10 observations) }	:: 48 : 228.
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In the preceding experiments the centre of the pile was at a distance (along the radius) of 1.5 in. from the rim of the disk. In the following experiment it was adjusted so as to be at a radial distance of 2.9 in. from the rim of the same disk. With this alteration and with the thin aluminium disk we obtained

Effect for $\frac{3}{20}$ hyd. (mean of 7 observations) }	:	{ Effect for $\frac{3}{20}$ hyd. + $\frac{6}{20}$ air (mean of 9 observations) }	:: 37 : 175.
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We thus see that the effect for $\frac{3}{20}$ hyd. (which may be supposed to represent the residual effect) and that for $\frac{3}{20}$ hyd. + $\frac{6}{20}$ air (which may be supposed to represent the gas-effect) are both diminished in very nearly the same proportion, namely 100 : 77, by the above transference of the pile to a position nearer the centre of the disk. It would thus appear that the difference in distribution among the particles of the two effects brought to light in art. 44 cannot be explained by a difference from centre to rim, but would rather seem to be due to a difference in depth. If this appear to be improbable, it must be remembered that these experiments were not made on a naked metallic disk. These experiments would therefore appear to show that in an aluminium disk covered with varnish, as well as in a disk of ebonite, we may imagine the residual effect to be more deeply seated than the gas-effect.

46. We venture on the following as what appears to us to be the most probable explanation of the whole body of experiments, including those with radiation.

(1) There is a temporary heat or cold effect (art. 26) which

* May not this peculiarity of the aluminium disk in the hydrogen vacuum be due to some hygrometric surface-effect resident in the varnish? In one experiment, but only one, the peculiarity was absent.

may be supposed to arise in particles very slightly attached to the disk; this is radiated off chiefly during rotation, and probably does not greatly affect the disk afterwards.

(2) There is a surface gas-effect, which in an aluminium and even in an ebonite disk is conducted into the interior as it arises, so that it does not greatly radiate during rotation of the disk (art. 26). In a paper disk, however, which is formed of a badly conducting material loosely put together, part of the effect does escape as radiation during rotation (art. 27).

(3) There is a residual effect, which is more deeply seated than the gas-effect. And inasmuch as radiation takes place from a perceptible depth, this effect is much more influential than the gas-effect in increasing radiation after rotation. In the case of a paper disk, this deeply seated effect will be less diminished by radiation during rotation than the gas-effect, and therefore after rotation in such a disk we might expect the gas-effect to be peculiarly small (art. 35).

47. In the course of these experiments we have endeavoured to prove that this residual effect is not caused by vibration. The radiation-experiments with aluminium disks of three different thicknesses went, on the other hand, to show that it was of the nature of a surface-effect. This is confirmed by the results derived from tapping; for, in the first place, the experiments of art. 45 show that the two effects (the residual and the gas-effect) are probably distributed in the same proportion, going from the centre to the circumference of the disk. Again, taking the two disks of thickness .05 and .025 of an inch, we obtain the following results:—

	Effect for $\frac{3}{20}$ hyd.	Effect for $\frac{3}{20}$ hyd. + $\frac{6}{20}$ air.
Thin disk	48 (22 observations).	228 (10 observations).
Thick disk	29 (20 observations).	108 (10 observations).

Now, allowing for errors of experiment, we see that the residual, as well as the gas-effect, is reduced to about one half for the thick disk.

Again, an experiment of a similar nature gave the effect for $\frac{3}{20}$ hyd. in an ebonite disk of $\frac{1}{10}$ in. in thickness = 33, against a result = 55 for the thin ebonite disk. Unfortunately it was omitted to make a comparison with these two disks for the gas-effect; nevertheless these results are all in favour of the residual effect being a surface-effect.

48. It might be well to make one remark regarding these experiments. They are not like the radiation-experiments, which required an extremely delicate instrument in order to give a sensible effect. But, on the other hand, the effect obtained by tapping being that due to the mere surface of the disk, is liable to be altered by any thing which affects the surface of the disk. We have come to the conclusion that in such experiments it is unadvisable to use a porous hygrometric surface, such as that of paper, not having its pores filled with paraffin or some other similar substance. It is likewise desirable that all parts of the apparatus should be as nearly as possible of the same temperature; indeed we suspect that some ex-

periments made during some very peculiar summer weather were influenced by a want of temperature-equilibrium between the various parts of the apparatus, the result appearing to be that the gas-effect for the ebonite disk was abnormally large.

49. Our conclusion from the evidence before us is, that the residual effect is a surface-effect more deeply seated than the gas-effect, but distributed outwards from the centre to the circumference, very much in the same manner as the gas-effect. The residual effect likewise appears able to penetrate a chamois-leather blind without any perceptible diminution. We regard these conclusions as preliminary, and shall endeavour in our future experiments to procure additional evidence of these properties of the residual effect, as well as to obtain new facts regarding it. In the mean time, as the subject is one of interest, and has been already too long delayed, we have not hesitated to bring these results before the notice of the Royal Society.

In concluding we would desire to express our thanks to Mr. F. Kingdon for his assistance to us in many of these experiments.

June 19.—William Spottiswoode, M.A., Treasurer and Vice-President, in the Chair.

The following communication was read:—

“The Diurnal Variations of the Wind and Barometric Pressure at Bombay.” By F. Chambers.

The object of this paper is to bring to notice a remarkable relation that has been found to exist between the diurnal variations of the wind and the barometer at Bombay.

The observations made use of are the records of a Robinson’s anemograph during the first three years of its performance, viz. from June 1867 to May 1870, and the corresponding hourly observations of the barometer and the dry- and wet-bulb thermometer made at the Government Observatory, Bombay.

The mean results for each hour of the day during the whole period and the mean diurnal variations of each element are tabulated and graphically represented by figures. The diurnal variation of the wind is then investigated, the most influential part of which is attributed to the land- and sea-breezes which blow from E.S.E. and W.N.W., and are shown to follow mainly the same law of progression as the temperature of the air, thus affording confirmatory evidence of the truth of Halley’s theory of the trade-winds as applied to land- and sea-breezes.

Some peculiarities of the curve representing the land- and sea-breezes are then pointed out, and these, the writer concludes, are due to the superposition of another distinct variation having two maxima and two minima in the twenty-four hours, like the barometer variation; and he supports his views by a reference to the variation of the east components of the wind in the months of July and August, when the land- and sea-breezes have almost disappeared. This is found to exhibit a decided *double* period. The north components of the land- and sea-breezes are then approximately eliminated from the north components of the whole variation, and the

variation which then remains exhibits a very decided *double* period in this direction also. These variations with double periods are regarded as indicative of the existence of a double diurnal variation in the general movements of the atmosphere. Upon this hypothesis typical diurnal variations of the wind are deduced for north and south low latitudes—that for north latitudes exhibiting a double diurnal right-handed rotation, and that for south latitudes a double diurnal left-handed rotation; and from these the diurnal variation of the barometer is deduced.

The movements of the wind-vane at Bombay are then analyzed; and the writer concludes that the greater part of the excess of “direct” over “retrograde” rotation of the vane at Bombay is due to the *diurnal variation* of the wind.

Extracts are given from observations made at St. Helena, Toronto, and Falmouth, showing the character of the diurnal wind-variations at those places, and their greater or less agreement with the deduced typical curves. The writer maintains that these variations afford independently a possible, if not a probable, explanation of that movement of the air which Dove had called the “Law of Gyration;” and, in conclusion, he points to the extent of their applicability in deducing weather probabilities, and to the method of discussing storms.

A postscript is added, giving the mean diurnal variation of the wind at Sandwick Manse, Orkney, and pointing out its general conformity with the results deduced from the Bombay wind-observations.

GEOLOGICAL SOCIETY.

[Continued from p. 77.]

February 26, 1873.—Prof. Ramsay, F.R.S., Vice-President,
in the Chair.

The following communications were read:—

1. “On the Jurassic Rocks of Skye and Raasay.” By James Bryce, M.A., LL.D., F.G.S.

In this paper the author described numerous sections of Jurassic rocks exposed chiefly in the sea-cliffs of Skye and Raasay, indicating the presence in those islands of a complete series of beds ascending from the Lower Lias to the middle of the Middle Oolite. He noticed the occurrence in these sections of fossils belonging to the zones of *Ammonites angulatus* and *A. Bucklandi* in the Lower Lias, to the zones of *A. Jamesoni*, *A. capricornus*, *A. margaritatus*, and *A. spinatus* in the Middle Lias, of Upper Lias fossils, including *Ammonites communis*, *falcifer*, *heterophyllus*, and *bifrons*, and of others indicating beds belonging to the Inferior Oolite and Cornbrash and to the Oxford Clay. The Loch-Staffin beds were described as an estuarine series, nearly approaching the Oxford Clay in geological age, and including a bed almost entirely made up of the shells of *Ostrea hebridica*.

The whole series of Jurassic rocks in these islands reposes on the Torridon sandstone of Cambrian age; and the author discussed the

question whether or not the intervening beds have ever existed in this locality, and came to the conclusion that they probably existed, and have been swept away by denudation. He remarked further upon the resemblance in lithological characters of the beds described to the corresponding deposits elsewhere in Britain. The trap-rocks intruded between the Jurassic deposits he regarded as of post-oolitic date. The paper was accompanied by lists of fossils and descriptions of some new species prepared by Mr. Ralph Tate.

2. "Observations on the more remarkable Boulders of the North-west of England and the Welsh Borders." By D. Mackintosh, Esq., F.G.S.

In this paper the author described the situation and indicated the probable origin of many of the more striking known boulders in Westmoreland, Cumberland, Lancashire, Cheshire, and on the borders of Wales. The northern boulders seem to have originated chiefly from Wastdale Crag, Criffel, Ennerdale, and Eskdale; those of Cheshire chiefly from the Lake district and south of Scotland; and many of those on the Welsh borders from the mountains of Wales. Many of the boulders noticed by the author exhibit glacial striæ. The author also especially referred to the occurrence of boulders at high levels.

March 12th, 1873.—Joseph Prestwich, Esq., F.R.S., Vice-President, in the Chair.

The following communications were read :—

1. "Note on some Brachiopoda collected by Mr. Judd from the Jurassic Deposits of the East Coast of Scotland." By Thomas Davidson, Esq., F.R.S., F.G.S.

In this note the author stated that four species of Brachiopoda collected by Mr. Judd were especially worthy of notice, two of them being quite new, and two new to Britain. Three of them were obtained from the equivalent of the Kimmeridge Clay, which was the more remarkable as the Brachiopoda of that formation are comparatively few. The new species described were *Rhynchonella Sutherlandi* and *Terebratula Joassi*, derived, with *Terebratula humeralis*, Röm., from the Upper Oolite of Garty in Sutherland; the fourth species is *Terebratula bisuffarcinata*, Schlot., from the Lower Calcareous Grit of Braamerry Hill. This note is intended to form an appendix to Mr. Judd's paper on the Secondary rocks of the east coast of Scotland, and is accompanied by a plate.

2. "On Solfataras and deposits of Sulphur at Kalamaki, near the Isthmus of Corinth." By Prof. D. T. Ansted, M.A., F.R.S., F.G.S.

After noting the traces of volcanic action east of the Pindus chain, the author described the Solfataras and sulphur-deposits of the neighbourhood of Kalamaki as furnishing indications that there is even now a real though subdued volcanic energy in this part of Europe. At this place, about three miles east of the Isthmus of Corinth, there is a series of cream-coloured and grey gypseous marls, broken

by narrow gorges and fissures. These marls, the stratification of which is much disturbed, are loaded with sulphur. In the principal gorge there are several lateral fissures, forming caverns, communicating with the interior by deep cracks; these caverns are completely lined with crystals of sulphur and other volcanic minerals, and are rendered inaccessible by the large body of hot stifling vapours constantly emitted through them. The temperature of this vapour, where it can be reached, is about 100° F.; but the floor of the caverns is too hot to stand on, even near the entrance. The author is of opinion that the rocks in this gorge might be profitably worked for sulphur. Similar phenomena occur in several other places within about a mile of the ravine described; and the author was informed that this was the case also several miles further to the east.

The author adverted to other signs of volcanic action to the west of these solfataras, and especially to the structure of the Acropolis of ancient Corinth, and inferred that lines of volcanic action parallel to the spurs of the Alps of which Etna and Vesuvius and Santorin are the modern vents, ranged far to the north at no distant period.

3. "On the Origin of Clay-Ironstone." By J. Lucas, Esq., F.G.S.

The author commenced by giving a general view of the varieties, chemical composition, and mode of occurrence of Clay-ironstone, and suggested that the formation of all the bedded varieties may be explained by the supposition that they originated in peaty or non-peaty lagoons on the alluvial flats of the deltas of the Carboniferous formations, which would present semiterrestrial conditions—that is to say, a surface exposed to the air, but subject to be covered by floods. Carbonic acid (formed in the lagoons from decomposing vegetable matter) meeting with protoxide of iron in solution, would unite with it to form carbonate of iron, which, with the mud of the lagoon, would produce Clay-ironstone. Thus, in the author's opinion, the beds of Clay-ironstone, like Coal-beds, mark terrestrial horizons. The author supported his views by reference to various sections, and also cited the occurrence of what he regarded as an analogous phenomenon on a small scale in some mud obtained from the shore between Redcar and Saltburn.

4. "Note in vindication of *Leptophlæum rhombicum* and *Lepidodendron gaspianum*." By Principal Dawson, LL.D., F.R.S., F.G.S.

This note accompanied some photographs of the remains of plants referred to, and was in opposition to the identification of these remains with the *Lepidodendron nothum*, Unger, as proposed by Mr. Carruthers in his Appendix to Mr. Daintree's paper on the Geology of Queensland. The author maintained that his *Lepidodendron gaspianum* is distinct from Unger's species, although nearly allied to it, and that his *Leptophlæum rhombicum* differs from both generically and specifically. He tabulated as follows the characters of his two species:—

	<i>Lepidodendron gaspianum.</i>	<i>Leptophlœum rhombicum.</i>
Stems and branches...	Long and slender; areoles elongate lanceolate.	Short and stout; areoles regularly or transversely rhombic.
Vascular scars	In middle of areole or nearer upper end, according to the surface exposed.	Always in middle of areole.
Leaves	Short and much curved outward.	Long and somewhat straight and erect.
Fruit.....	Small, scaly.	Long, leafy.
Structure	Unknown; probably allied to Carboniferous <i>Lepidodendra</i> .	Known to possess a large <i>Sternbergia</i> -pith, and to have a very thin cortical layer.

The author further stated that Presl's genus *Bergeria* is Carboniferous and not Devonian, and that it has no special affinity with *Leptophlœum* or *Lepidophloios*; and he objected to the revival of the name *Bergeria*.

March 26th, 1873.—His Grace the Duke of Argyll, K.T., F.R.S., President, in the Chair.

The following communications were read:—

1. "Synopsis of the younger formations of New Zealand." By Capt. F. W. Hutton, F.G.S., of the Geological Survey of New Zealand.

In this paper the author gave a summary of the Tertiary and later Secondary formations of New Zealand. He stated that he had been able to determine 375 species of true Mollusca, 12 of Brachiopoda, and 18 of Echinodermata from the Tertiaries; and under each of the formations which he recognizes he gave the number of species of true Mollusca found in it, indicating the number of recent species and of those belonging to other formations occurring in each. He also noticed the range and distribution of the various formations. The Tertiary groups of strata distinguished by the author are, in descending order, as follows:—I. Pleistocene. II. Pliocene: 1, the Newer Pliocene or Whanganui group; 2, the Older Pliocene or Lignite group. III. Miocene: 3, Upper or Arvatere group; 4, Lower or Kanieri group. IV. Oligocene: 5, Upper or Hawke's Bay group; 6, Lower or Waitewata group. V. Eocene: 7, Upper or Ototara group; 8, Lower or Brown-coal group. As belonging to the Mesozoic series, the author also described beds of Danian age, under the name of the Waipara formation. A species of *Belemnitella* occurs in beds belonging to the Ototara group, and also in the Waipara formation. Volcanic action commenced in the North Island during the deposition of the Waitewata group, and has since been almost continuous in the northern, western, and central parts of the island. In the South Island the volcanic formations appear to belong to the later Cretaceous, Oligocene, and Miocene periods. The volcanic rocks of the Chatham Islands belong chiefly to the Upper Oligocene.

2. "On the Tree Ferns of the Coal-measures, and their relations to other living and fossil forms." By W. Carruthers, Esq., F.R.S., F.G.S.

After referring to the remarkably uniform character of the order of Ferns throughout their whole history on the globe, the author pointed out that there existed in the Coal-measures two very distinct kinds of fern-stems, each represented by several species. Both of these were very different from the *Chelepteris*-group already described by the author in the Journal of the Society. The first group had a stem-structure like that of living tree ferns. In them the vascular elements of the stem formed a closed cylinder round the pith; and the vascular bundles for the leaves were given off from the out-turned edges of the cylinder, where, at regular intervals, corresponding to the position of the leaves, narrow meshes occur for this purpose. To this group were referred the stem described by Lindley and Hutton as *Caulopteris Phillipsii*, and several hitherto undescribed species from Radstock and Newcastle. No materials had yet been detected which could throw any light on the foliage or fruit of these fern-stems. The second group included some stems the casts of which the author had obtained from Radstock, and the root-structures from Halifax. By the help of a fine series of specimens in the collections of the British Museum, he was able to correlate the different parts of these plants. The stems had been described by Corda under the name of *Stemmatopteris*. They differed from the other group chiefly in having the ends of the vascular plates, as seen in the transverse section, turned inwards, and having the bundles of the leaves formed in a complete condition in the axis of the stem. The author showed that the relation of the different parts of the stem in the species of *Caulopteris* was the same as in a first year's dicotyledon, while in the latter group the analogy of the structures was with the monocotyledonous stem. The roots, which surrounded the older portions of the stem, formed the well-known genus *Psaronius* of Cotta; and as this was the earlier name, it was proposed to retain it for the genus. There was associated with all the fine specimens of this group which he had received from J. M'Murtrie, Esq., F.G.S., foliage which had been described as *Cyatheites arborescens*. Although this had not been observed organically connected with the stems, the author adduced several reasons for believing that it belonged to them. If this connexion could be established, it was of the greater importance, as this form was known in fruit, and the fruit established that its affinities were with the living *Alsophilas* and *Cyatheas*. Many species of this genus occurred in the continental coal-fields; but the author believed that all the specimens found in England, though differing considerably amongst themselves, belonged to a single species.

3. "Notes on the Geology of Kazirùn, Persia." By A. H. Schindler, Esq.

In this paper, which accompanied a series of specimens presented to the Museum of the Society, the author described the section presented by the hills in the neighbourhood of Kazirùn. The general

surface was described as consisting of nearly unfossiliferous Post-tertiary deposits, immediately beneath which is an unstratified marine deposit containing a great abundance of fossils, among which are species of *Ostrea*, *Pecten*, and *Cidaris* (?). Below this deposit is a succession of strata, repeated several times in the hills; and at the bottom of the series in each case is a bed of gypsum. The spaces between the recurrent series are filled up with conglomerates. Beneath the gypseous series is a formation of compact limestone, which rises to a height of about 1500 feet both north and south of the plain of Kazirùn; its beds dip 25° , and their strike is from N.E. to S.W. The author mentioned the occurrence in a gypsum quarry near Kazirùn of three long cylinders from 6 to 10 feet in diameter, composed of the same material as the surrounding rock. He also stated that he had examined several caverns in the neighbouring mountains, and also the great cave at Shahpoor, but without discovering any organic remains.

April 9th, 1873.—His Grace the Duke of Argyll, K.T., F.R.S.,
President, in the Chair.

The following communications were read :—

1. "Lakes of the North-eastern Alps, and their bearing on the Glacier-erosion Theory." By the Rev. T. G. Bonney, M.A., F.G.S.

The purpose of this paper was to test, by the Lakes of the Salzkammergut and neighbourhood, the theory of the erosion of lake-basins by glaciers, which has been advanced by Professor Ramsay. The author premised :—

(1) That an extensive glacier could not exist without a considerable area to supply it.

(2) That under no circumstances could a glacier excavate a cliff of considerable height (say 1000 feet) approximately vertical.

(3) That owing to the proximity of the regions, a theory of excavation which applied to the Western and Central Alps ought to be applicable also to the Eastern Alps.

He then proceeded to examine a number of lakes in detail.

The Königsee lies in a remarkably deep, steep-sided valley, terminated by a cirque, with cliffs full a thousand feet high, and has no large supply-area behind.

The Hallstättersee is similarly situated, has a cirque at the head, and two lateral valleys nearly at right angles to the lake, up which arms of it have formerly extended. These are not likely to have furnished glaciers which could have excavated the lake; and above the cirque there is no large supply-area.

The Gosautal consists of lake-basins separated by valleys of river-erosion.

The Fuschelsee and Wolfgangersee, on the south side of the Schafberg, are separated by a narrow sharp ridge of hills, incapable of nourishing glaciers large enough to grind them out; there are no signs of glaciers from other directions having eroded them.

The Mondsee and Attersee (once one lake) on the north lie under the steep cliffs of the Schafberg, which could not have nourished a large glacier; and the ridge of the Schafberg is too sharp

to admit of the supposition that a great glacier, coming from the south, has passed over it to excavate the lake; yet the Attersee, in a position least favourable to glacial action, is the largest and deepest lake in the Salzkammergut. The head of the valley in which these lakes lie is really among low hills, in the direction of the Austro-Bavarian plain.

The Traunsee was shown to give no evidence in favour of a theory of glacial erosion.

Since, then, these lakes either had at their heads preglacial cirques (the very existence of which was incompatible with much erosive power on the part of a glacier), or were beneath sharp and not greatly elevated ridges of rock, the author concluded that they had not been excavated primarily by glaciers.

He considered a far more probable explanation to be, that the greater lake-basins were parts of ordinary valleys, excavated by rain and rivers, the beds of which had undergone disturbances after the valley had assumed approximately its present contour. He showed that the lakes were in most cases maintained at their *present* level by drift,—and that while in a region so subject to slight disturbances as the Alps positive evidence for his theory would be almost impossible to obtain, no lake offered any against it, and one, the Königssee, was very favourable to it.

2. “On the Effects of Glacier-erosion in Alpine Valleys.” By Signor B. Gastaldi.

The author described the occurrence in the valley of the Lanzo and other Alpine valleys, at heights between 2000 and 3000 metres (6700 and 10,000 feet), of large cirques, in two of which, in the valley Sauze de Césanne, the bottom was occupied in the autumn by glaciers reduced to their smallest dimensions. The author noticed the various rocks in which these cirques were cut, and expressed his opinion that they are the beds formerly occupied by glaciers, the power of which to excavate even comparatively hard rocks, such as felspathic, amphibolite-, and chlorite-schists, he considered to be proved.

The author then referred to the mouths of the Alpine valleys opening upon the plain, which he described as being generally very narrow in proportion to their length, width, and orographical importance; and he pointed out that in the case of the valley of the Stura, at any rate, the outlet of the valley has been cut out by the river. This peculiarity he accounts for by the fact that whilst the calcareous and felspathic rocks are easily disintegrated by atmospheric action, certain other rocks, such as the amphibolites, diorites, syenites, amphibolite-schists, euphotides, serpentines, &c., resist atmospheric denudation; and he indicated the peculiar distribution of these rocks in the region under consideration, by reason of which portions of them occupied the points which are now the mouths of the valleys.

XXX. *Intelligence and Miscellaneous Articles.*

ON THE LIMITS OF THE EFFECTIVE POWER OF MICROSCOPES.

BY PROFESSOR HELMHOLTZ.

ACCORDING to a general law of optical instruments, first advanced by Lagrange, the more the magnifying is augmented the narrower become the pencils of rays which pass from a single point of the object through the instrument. The narrower the cones of rays, the less becomes the brightness of the image, and the greater the indistinctness occasioned by entoptic shadows and diffraction. If the magnitude of the smallest perceptible objects is estimated from the distance between each two bright lines which can be recognized as separate from one another, this magnitude may be regarded as equal to that which in the magnified image of the object equals the width of the outer diffraction-fringes of each bright point. This is a quantity dependent only on the angle of divergence of the incident rays, and independent of the construction of the instrument.

Calling α the angle formed with the axis by the extreme rays of the pencil from the axial point of the object, incident on and passing through the instrument, at their starting-point, λ the wave-length of the light in the medium in which the object is situated, ϵ the magnitude of the smallest recognizable distance on the object,

$$\epsilon = \frac{\lambda}{2 \sin \alpha}.$$

If the rays pass through a plane surface, in air, perpendicular to the axis, and if the values of λ and α referred to air be denoted by λ_0 and α_0 , we can also write

$$\epsilon = \frac{\lambda_0}{2 \sin \alpha_0}.$$

Our newer immersion microscopes make α_0 nearly equal to a right angle; then ϵ becomes equal to half the wave-length of the light employed. For the middle greenish yellow light of the greatest brightness we can put

$$\lambda_0 = 0.00055 \text{ millim.},$$

$$\epsilon = 0.000275 = \frac{1}{3636} \text{ millim.}$$

The more reliable of the newer measurements, especially those made on objects which actually give a broad cone of light, have for their result numbers (as $\frac{1}{3312}$ millim., Harting) not much greater than that.—*Monatsbericht der kön. preuss. Akad.* September and October 1873, p. 625.

EXPERIMENTAL RESEARCHES ON NEWTON'S COLOURED RINGS.

BY P. DESAINS.

The experimental verification of the theory of Newton's rings ordinarily consists only in measuring, under one or several incidences, the diameters of a certain number of those rings. A good micrometric screw permits the measurements to be made very conveniently and with much rigour. A screw can also be employed for measuring directly the quantity by which the distance between

the two glasses must be increased to force the ring of the order n to come and take the place of the central spot, and that in the different prismatic colours. According to the theory this quantity is equal to n times half the wave-length of the light employed. The apparatus used by me, in the laboratory of instruction of the Faculty, to verify this point of the theory, was constructed with much care by M. Laurent-Duboscq.

The rings are formed as usual, between a lens and a plane. The lens is fixed, or has only revocable movements; the plane moves with regularity under the action of a good micrometric screw, the channel of which was ordered equal to half a millimetre. Now, if some parts quite at the extremities of this screw be excepted, whatever the part which is found in the nut, we may assure ourselves at first that the angular course necessary for causing sixty consecutive rings to enter into or issue from the central spot is constant.

This first point verifies the exactness of the screw, and that, as is evident, by a process simple, rapid, and, it seems to me, capable of being applied in many cases.

Secondly, I have observed that this angular course corresponding to sixty rings is $12^{\circ}85$.

We have, then, if the channel is quite $\frac{1}{2}$ a millim.,

$$60 \frac{\lambda}{2} = 0.5 \frac{12.85}{360} \text{ millim.},$$

whence we deduce

$$\lambda = 0.000594 \text{ millim.}$$

The light employed was that of gas salted; and it is pretty generally admitted that the wave-length of the rays in the vicinity of the line D is 0.000589.

It is easy to perform analogous experiments by illuminating the glasses with sensibly homogeneous rays from the different regions of a good spectrum; and I have ascertained that the spectrum of a Drummond lamp is quite intense enough for this purpose.

The number of rings which pass with an entire turn of the screw is 1683 when the operation takes place with the light in the vicinity of the line D. Now, as the button with which the action is performed is only 4 centims. in diameter, it is clear that the hand must be moved very slowly in order that several rings at once may not be made to enter the central spot. When we wish to limit ourselves to observing the passage of fifteen rings only, it is preferable to conduct the principal motor screw with a tangent micrometric screw itself. In my apparatus, an entire turn of the tangent screw corresponds to only two and a half rings; consequently one can easily divide into more than 100 equal parts the movement of the ring which answers to a displacement of 0.000592 in the movable glass.—*Comptes Rendus de l'Acad. des Sciences*, January 26, 1874.

ON EXTRAORDINARY DEVELOPMENTS OF LIGHT IN THE GRINDING
OF HARD STONES. BY DR. JACOB NÖGGERATH.

It is known that a number of minerals and stones become phosphorescent by friction, especially on being rubbed against each other

in the dark. This is particularly the case with hard siliceous stones, such as rock-crystal, quartz, chaledony, &c. The phenomenon takes place both in air and under water. But only the surface of the stones appears to phosphoresce. For many years, however, I have known that under very powerful friction, such as can only be produced by corresponding machinery, such stones become throughout splendidly luminous with red light and at the same time transparent. This takes place at the agate-polishing establishments at and near Oberstein and Idar in the Oldenburg principality of Birkenfeld-on-the-Nahe. It has not been expressly mentioned by any writer on the agate industry of that region. Collini, who a century ago described with great accuracy all the arrangements and manipulations in operation there, just as they exist unaltered at the present time, makes only the general remark that polishing agates on the rapidly revolving grinding-stones produces "fire and sparks."

On a recent visit, for a mineralogical purpose, to Oberstein and Idar, I made some observations on these phenomena, which I will here communicate.

The establishment is situated on a water-course, which drives an undershot water-wheel, the axle of which reaches into the room, and here, by means of two cog-wheels, moves a horizontal shaft, on which are four or five grindstones rotating vertically. Over each is brought a gutter so that a constant small stream of water pours over the rotating stone and between it and the stone to be ground.

The grindstones consist of very solid fine-grained sandstone from the Bunter formation, and are obtained in the neighbouring Rhenish Palatinate. They must be faultless throughout, without flaws, clay-cells, and such like, as otherwise they are very likely to crack with the rapid rotation. There have been many instances, even recently, of stones (which had unobserved defects) cracking when in rotation, flying asunder in fragments, killing the workmen, and making great havoc in the work-room, even shattering the roof and the walls. The stones are from 5 to $5\frac{1}{2}$ feet in diameter, and 14 inches thick at the grinding-surface. The average velocity is three revolutions in a second, therefore 180 in a minute, or 10,800 in an hour. Hence the grinding-surface that passes over the object pressed against it amounts to from 169,646 to 186,613 feet or from 7 to 8 (German) geographical miles (28 to 32 English) per hour.

When grinding, the workman lies with his belly and partially his chest on a semicylindrically hollowed-out stool raised somewhat obliquely in exactly the direction of the grindstone, his feet stretched out behind and resting against a post fixed in the floor. In this position he presses the stone to be polished, with both hands, firmly against the grindstone. With his head a little raised he can see to the placing of the object on the grindstone and to perform the entire operation properly. This position gives him the necessary purchase. As the muscular exertion of the workman is very great, the polishing is performed with intermissions, so that hours of work alternate with equal times of rest mostly employed in the preliminary operations.

The further working of the agates (boring, colouring, polishing,

&c.) I omit to describe, as these manipulations do not touch my purpose*.

My experiments on the luminous phenomena in question were performed in bright daylight at noon, the temperature of the air being about 14° R. I had only one hour for them, and could only make use of the few sorts of stone that were at hand.

The phenomena I observed were essentially of two kinds. As soon as a stone of about the hardness of quartz was pressed against the revolving grindstone, there was produced between them an intense red light, which at the same time radiated round the object and emitted numerous sparks. This appearance was the same with all hard stones.

The second phenomenon occurred simultaneously with the other, but only with translucent and transparent stones, not with completely opaque ones. The stones of the former quality shine with brilliant red light slightly yellow-tinged. They appear almost like red-hot iron; and it really looks as if the hands of the grinder must be severely burned. All the stones tried became warm in grinding, but not very hot; feeling them with my hand, I estimated the increase of temperature at from 10° to 12° R.

The stones on which I experimented in this way were the following:—

Chalcedony of a yellowish grey colour, a little translucent, from Uruguay, South America; a piece 2 inches thick became magnificently fiery red, and therewith transparent.

Chalcedony, white and opalescent, said to be oriental; behaviour as in the preceding experiment.

Chalcedony of a reddish colour, translucent, from Idar; gave the same result.

Chrysoprase from Silesia; a thick piece, pale green, very slightly translucent at the edges, hardly useful as a jewel, gave a little red light.

Rock-crystal, colourless, perfectly transparent, from Brazil. The luminous appearance was very magnificent; but the fiery red, in comparison with the above experiments, was more subdued, almost rose-red.

Rock-crystal, smoke-grey, transparent (the so-called smoky topaz), from the Swiss Alps. Pretty nearly the same result, only somewhat less beautiful.

Carnelian of a beautiful red colour, intensely translucent, from India; gave the most magnificent red light, as the colour of it was saturated with the natural colour of the carnelian.

Amethyst, crystals, intensely translucent, from Idar. The light was pale violet, because here also the violet-blue colour of the stone blended with the fiery red of the light, beautifully transparent.

Of perfectly opaque stones were tried red agate jasper from Idar, black lydite with thin white stripes of quartz, artificially black-coloured chalcedony from Uruguay, opaque heliotrope from India,

* A very precise account, illustrated with figures, will be found in Collini, *Journal d'un Voyage qui contient différentes observations minérales*, &c. Mannheim: 1776.

and, lastly, unweathered melaphyre from the Oberstein Railway Station. Within none of these stones was any light perceptible; only the light on the ground surface remained constant.

It deserves to be mentioned that J. H. Pl. Heinrich, as stated in his copious work on phosphorescence*, has made similar observations with far inferior appliances. For his experiments (which were all made in the dark) he employed an ordinary grindstone of $14\frac{1}{2}$ inches diameter, turned by hand, and making two revolutions in a second. The extent of surface, therefore, which in one second passed over the object applied to the grindstone was only 7 feet 7 inches, while in the agate-grindery it was from 47 feet 1 inch to 51 feet 10 inches. He thereby caused onyx, carnelian, chalcedony, rock-crystal, &c. to shine with fiery red light. He does not state, however, whether the shining was only superficial, or penetrated to the interior of the stones; and just as little does he say that under grinding they appear transparent. On the contrary, he says that the light is not so bright as when two stones of the same sort are rubbed against one another. Hence the light obtained by him can only have been very faint. He alleges, however, that a luminous arc appeared round the circumference of the grindstone. This I have not seen; and it is perhaps only to be observed in the dark.

If we inquire after the fundamental causes of the phenomenon of the illumination of the grinding surface, these may well be no other than the combination of friction and the development of electricity, of which the rise of temperature of the stone is a necessary consequence. The red halo round the stone which is being ground, and the scattering of sparks, can only arise from the illuminated particles separated from the two stones by the friction.

The second phenomenon, the magnificent fiery red illumination of the stone, seems to be a mere consequence of the radiation of the light through from the surface of contact of the stone to be ground and the grindstone, although transparent and merely translucent stones of 5 or 6 inches length are perfectly alike in diffusing the fiery red light. Hence also no light can be produced in completely opaque stones.

I was not able to make closer investigations. Much more might be said in respect of my experiments from the physical point of view. In the first place, immediately after the grinding of the stones, electroscopic experiments and trials of their increase of temperature might be made; also spectral analysis of the light would be of importance; lastly, my investigations should be supplemented by such with many other kinds of stone.

The intention of this publication of my experiments is substantially this—to call attention to the singular opportunity offered by the numerous agate-grinderies of Oberstein and Idar for the purpose in question, and to invite physicists to further investigations in these localities. I know not, being in my eightieth year, if I am destined to be able to complete my experiments.—Poggendorff's *Annalen*, 1873, No. 10, pp. 325–331.

* *Die Phosphoreszenz der Körper unter allen Umständen untersucht und erläutert*: Nürnberg, 1820, p. 510 seqq.

THE
LONDON, EDINBURGH, AND DUBLIN
PHILOSOPHICAL MAGAZINE
AND
JOURNAL OF SCIENCE.

[FOURTH SERIES.]

APRIL 1874.

XXXI. *The Vermiculites; their Crystallographic and Chemical Relations to the Micas; together with a discussion of the Cause of the Variation of the Optical Angle in these Minerals.* By JOSIAH P. COOKE, Jun., *Erving Professor of Chemistry and Mineralogy in Harvard College*.*.

[With a Plate.]

To Professor Tyndall, F.R.S.

MY DEAR PROFESSOR, Chemical Laboratory of Harvard College,
Cambridge, Feb. 15, 1874.

I SHALL send you by this mail the advance sheets of a paper of mine to be published in our Academy Proceedings, giving an account of some new observations, which I am sure will greatly interest you. The mineralogical details of the paper will not probably appeal to you; but they also have importance as bearing on the optical question. You will find that I have succeeded in constructing uniaxial crystals of the hexagonal type from rhombic crystals of 60° and 120° angle, and have also been able to show that, in some cases at least, hexagonal crystals are so constructed in nature. You will find, further, that I have been able to reproduce the optical conditions of a quartz crystal, and in such a way as to be able to point out the mode of molecular structure by which the result is produced. It would be unnecessary to recapitulate these points, which you will find fully stated in my paper, were it not that you might otherwise be repelled by the mineralogical crust with which the optical results are necessarily enclosed.

* From the Proceedings of the American Academy of Arts and Sciences, read December 9, 1873. Communicated by the Author.

Phil. Mag. S. 4. Vol. 47. No. 312. April 1874.

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In order that you may have the evidence on which my conclusions rest, I send several specimens :—

1. A plate of magnesian mica (phlogopite) from Jefferson Co., New York, with optical angle of about 14° .

2. Macle of the same, showing planes of optical axes inclined to each other, on different parts of the plate, at an angle of 60° , and also showing variation of optical angle in passing from one part of the plate to the other.

3. Compound plate essentially uniaxial, made by splitting plate like No. 1 and piling up the laminæ in alternating positions at angles of 60° .

4. Plate of Muscovite mica with optical angle between 60° and 70° .

5. Compound plate showing left-handed circular polarization, formed from last by piling thin laminæ ($\frac{1}{400}$ of an inch about) in left-handed spirals.

6. Compound plate showing right-handed circular polarization, formed from films of the same mica as 4 by piling in right-handed spirals. By placing 6 on 5 (point marked *a* on *a*) with the edges of the glass of 6 parallel with the pencil lines on 5, you will see, in certain azimuths with the plane of polarization, the Airy spirals beautifully reproduced.

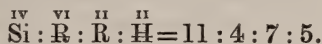
I am sorry that the last two specimens (by far the most interesting) are not more perfect. Neither of them presents constant features at different azimuths with the plane of polarization; but No. 5 is more perfect than No. 6; and a comparison of the two will show how I have approximated to the perfect result in proportion as I have succeeded in making the laminæ at the same time sufficiently thin and of uniform thickness. Plates as good as No. 5 can be readily prepared; but to do better than this, except by accident, involves selecting the laminæ by actual measurement with a spherometer; and since the laminæ must also be taken from one and the same piece of mica and cut in parallel positions, you can easily see that, even after succeeding in cleaving the mica into sufficiently thin films, you may work for many days before securing at least twelve laminæ from the same film of uniform thickness. These specimens, however, will illustrate my paper, if not by what they are in themselves, at least by that to which they point and closely approach. My observations were made with a polarizing microscope designed by Des Cloizeaux and made by Soleil. The object is illuminated by a very strongly converging beam of polarized light; and the instrument brings into the field at once the two hyperbolas of a plate of topaz with an optical angle of over 80° . An instrument, however, which will include in the field the two hyperbolas of the plate of Mus-

covite mica I send will show the effects I have described with the specimens sent with this.

Introduction.—In the American Journal of Science, vol. vii. p. 55 (1824), T. H. Webb described a mineral from Millbury, near Worcester, Mass., which has since been a mineralogical curiosity on account of its singular reaction when heated. The mineral consists of "small foliated scales distributed through a steatitic base. . . . When heated, it exfoliates prodigiously, the scales opening out into long worm-like threads made up of the separate laminae. Exfoliation commences at 500° to 600° Fahr., and takes place with so much force as often to break the test-tube in which the mineral may be confined. Before the blow-pipe it fuses at 3·5 to a greyish-black glass." It was named by Webb, as he says, "from the Latin *vermiculor*, 'I breed worms.'" The hardness of the mineral is 1–2, the specific gravity 2·756, the lustre talcose, and the colour greyish, somewhat brownish. It was analyzed by Crossley, who "separated with great care from the base the scaly mineral, which is the true vermiculite;" and his results were as follows:—

	1.	Oxygen, true.		Ratio, app.	2.	Oxygen, true.		Ratio, app.
Silica	35·74	19·06	19·06	11	35·74	19·06	19·06	2
Alumina	16·42	7·65	7·65	4	16·42	7·65	10·99	1
Ferric oxide . .	..	..	..	..	11·13	3·34		
Ferrous oxide	10·02	2·23	13·21	7				
Magnesia. . . .	27·44	10·98			27·44	10·98	10·98	1
Water	10·30	9·16	9·16	5	10·30	9·16	9·16	1
	<u>99·92</u>				<u>101·03</u>			

The results of analysis in column 1, and the portions of the description of the mineral in quotation-marks, above, have been taken from Dana's 'System of Mineralogy,' fifth edition, p. 493; and the atomic ratio which is there deduced is



In this analysis, however, Crossley could not have determined the state of the iron, which in the specimen I have examined is almost wholly in the ferric condition. If now we assume that the whole of the iron belongs with the sesquioxide radicals, the analysis would appear as in column 2, and the atomic ratio is then seen to be 2 : 1 : 1 : 1, which is undoubtedly the correct result.

In the year 1851, Mr. W. W. Jefferis, of West Chester, Pa., discovered at the ripidolite locality near that town a peculiar micaceous mineral which exfoliates like the Millbury vermicu-

lite, but which, instead of occurring in small laminæ, is found in large hexagonal plates. This mineral was analyzed by Professor Brush, and, although at first referred by him, "with a query," to vermiculite (*Amer. Journ. Sci.* (II.) vol. xxxi. p. 369, 1861), was subsequently described as a new species (*Amer. Journ. Sci.* (II.) vol. xli. p. 248, 1866), and named Jefferisite.

Several years later, Mr. John Hall, now of Philadelphia, sent to me for examination some rough six-sided prisms of a micaceous mineral which he had discovered at East Nottingham, in Chester County, Pa. This mineral also exfoliates when heated. It is a new species; and I have named it, after the discoverer, Hallite.

A year since I received from Colonel C. W. Jenks, in connexion with other minerals from his corundum mine on the Culsagee river, in Macon County, N.C., a specimen of still another micaceous mineral having the same remarkable pyrognostic properties. It proved to be the best defined of any of this class of minerals which I had examined; and I shall designate it as Culsageeite.

Besides the above, there have been found several other micaceous minerals whose pyrognostic and crystallographic characters indicate that they belong to the same family, but which have not yet been investigated.

The remarkable exfoliation and great apparent increase of volume which the class of minerals under consideration undergo when heated are analogous to the well-known phenomena presented in the dehydration of alum, borax, and other crystalline salts when heated in a similar way; and it will be one object of this paper to show that the effect is due to the same cause—namely, to the escaping of what we call water of crystallization. I also expect to show that the several minerals referred to above are members of a family of hydrous silicates closely allied and parallel to the well-known family of anhydrous silicates called the micas, and that their molecules differ from those of the magnesian micas chiefly in containing a definite number of molecules of water—that is, water of crystallization. I shall call this family of minerals "the vermiculites," using the original name, as "mica" is now employed, to designate a class; and I shall call the three species (or varieties?) of this family Jefferisite, Culsageeite, and Hallite, which correspond, as I expect to show, to the two varieties of Biotite and to phlogopite respectively. It will appear that the original vermiculite has the same composition as the variety from the Culsagee mine. Finally, attention will also be asked to some unexpected discoveries to which the optical examination of these minerals has led.

Water of Crystallization and Water of Constitution.—In the

salts of the stronger acids the distinction between water of constitution (basic water as it is sometimes called) and water of crystallization is, as a rule, well marked. Water of crystallization generally escapes at a comparatively low temperature, and the loss is attended with a complete disintegration of the crystals, which usually fall to powder. Water of constitution is held more firmly, and the salt must be heated to a much higher temperature before it is expelled. Moreover, although the loss of basic water may greatly alter the external aspect of the body, yet there is not the same complete breaking up of the structure as before. To illustrate these points, it is only necessary to refer to the well-known reactions of the common rhombic sodic phosphate when heated.

It is generally believed that the water of crystallization exists as such in the salt, the molecules of water forming a part of the structure of the crystals; and the facts to be presented in this paper tend to support this view. But our modern theories assume that the so-called water of constitution, which may be also driven off when the salt is sufficiently heated, is formed under the influence of the heat from the atoms of hydroxyl, H-O-, which are united to the acid radical of the compound. But in every simple salt, whatever may be our theories in regard to the mode of combination, the weight of the water of constitution, as well as that of the water of crystallization, must bear to the weight of the salt a definite proportion, which can easily be calculated from the symbol; and the ratio between the amount of hydrogen and the amounts of the other radicals must be that of the atomic weights of these radicals or of their multiples. In any given class of salts, moreover, in which the ratio between the atomicity of the basic and acid radicals has a constant value, all hydrogen which represents water of constitution will supplement the other basic radicals, and added to them will complete the relative amount of basic radical which the given class of salts require; while the hydrogen which represents water of crystallization will be in excess of that amount.

In mineral silicates these relations are complicated by the phenomena of isomorphous replacements; and although there may be some question in regard to the replacing capacity of hydrogen, we cannot expect the same constancy, in the amount of water of constitution which these minerals contain, as in the case of simple salts. The amount of water of crystallization, however, must be as invariable in one case as in the other, and the hydrogen must bear the same relations to the atomic ratio of the compound. Thus, if the mineral is an orthosilicate, in which the atomic ratio between the *sum of the basic radicals* and the *silicon* is 1 : 1, all hydrogen in excess of the amount which

this ratio requires must represent water of crystallization, while all required to complete the ratio must represent water of constitution; and we thus have a means of distinguishing between these two states of combination where the class of compounds to which the mineral belongs is known.

Now it is true of each of the minerals we have distinguished among the vermiculites,—

First. That the water is driven off at a temperature below a red heat.

Secondly. That the loss of the water is attended with a complete disintegration of the mineral.

Thirdly. That the amount of the basic radicals, exclusive of the hydrogen, is sufficient to saturate the silicon, and that the amount of hydrogen is wholly in excess of the amount which the atomic ratio 1 : 1 requires.

Could it be proved that the vermiculites are orthosilicates, the last of the three facts just stated would be alone sufficient to establish the correctness of our conclusion. Unfortunately such absolute proof cannot be obtained; and we only claim that the crystallographic and chemical relations of the vermiculites to each other, and to the magnesian micas, give a very high degree of probability to our theory that they are orthosilicates.

From these facts we have concluded that the water which enters into the composition of the vermiculites is water of crystallization. For the evidence of the facts, we refer to the descriptions of the several species given below. But, further, in order to justify our conclusion, we propose to bring into comparison with the vermiculites a class of hydrous micas from which the water obtained is clearly water of constitution; and this class of minerals we shall describe first.

Damourite.—Delesse originally gave this name to a hydrous mica which occurs in fine scales in Pontivy in Brittany. Since then micas of similar composition have been observed in several countries, and shown to be not unusual constituents of granitic rocks*. Among these we may distinguish several varieties (or species?), marked by slight differences of composition and optical characters. But we would propose to give the name *Damourite* to the whole class, distinguishing the varieties by separate names only so far as may be thought necessary. Under the family of *Damourites*, then, we class all unisilicate micas, which are chiefly silicates of aluminium and potassium, but in which a portion of the alkaline radical is replaced by hydrogen.

Sterlingite.—A remarkable mineral of the *Damourite* type is found at Sterling, Mass., associated with spodumene, in a vein of a large boulder rock. This mineral, for the sake of distinc-

* See the papers of Professor Haughton, cited below.

tion, I have called *Sterlingite*; but it does not differ from the original *Damourite* of Delesse except in the value of the optical angle. We give in parallel columns the characters of the two minerals:—

Sterlingite.

Mica-like in structure. An aggregate of flexible and unelastic laminæ, frequently an inch in diameter.

Lustre, pearly.

Colour, yellow or yellowish white.

H=2-3.

Sp. gr. at 26°:—

1st ex. gave 2·832.

2nd „ 2·828.

Cleavage basal, highly perfect, as in mica. Jointed parallel to the sides of a rhomb, having an angle of 120°, and also parallel to the shorter diagonal of the same. The markings of these joints or cleavages visible on the surfaces of the laminæ.

Double refraction strongly negative.

Biaxial, with plane of axes parallel to shorter diagonal. Divergence about 70°. Dispersion of axes very small.

Before the blowpipe, fuses on the edges with potash-flame.

Damourite.

Mica-like in structure. An aggregate of fine scales.

Lustre, pearly.

Colour, yellow or yellowish white.

H=2-3.

Sp. gr. =2·792.

Double refraction negative. Biaxial divergence 10° to 12°.

Analysis.

<i>Sterlingite.</i>			<i>Damourite.</i>		
		Oxygen.	1.	2.	Oxygen.
SiO ²	43·87	23·40	45·22	43·41	23·15
Al ² O ³	36·45	17·00	37·85	35·17	16·39
Fe ² O ³	3·36	1·00		4·62	1·39
MgO				1·40	0·56
K ² O	10·86	1·84	11·20	10·90	1·85
H ² O	5·19	4·61	5·25	4·50	4·00
	99·73		99·52	100·00	

Of the two analyses of *Damourite*, No. 1 is by Delesse, of the mineral found in the gangue of cyanite, at Pontivy in Brittany, and No. 2 is by Igelström, of the similar mineral found at Horrsjöberg, Wermland. The analysis of *Sterlingite* was made by Mr. C. E. Munroe, Assistant in the Chemical Laboratory of

Harvard College; and the above numbers have been abundantly confirmed in repeated analyses by various students in the same laboratory. The alkalies were treated by Smith's process; and the potassium was weighed as PtK^2Cl^6 . This value, compared with the total weight of the alkaline chlorides and that of the chlorine, also determined, showed that the alkali in the mineral was almost wholly potash, although the presence of lithium and sodium was plainly indicated by the spectroscope. The water was determined by igniting the mineral in coarse powder, previously dried at 100°C . Even after ignition the finely pulverized mineral is only partially decomposed by hydrochloric acid; and in the above analysis it was decomposed by fusion with sodic carbonate. The usual tests failed to indicate the presence of fluorine. Regarding the water as basic, and as forming a part of the protoxides, the atomic ratio in Sterlingite between the silicon, the sesquioxide radicals, and the protoxide radicals, is

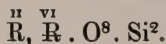
$$\overset{\text{IV}}{\text{Si}} : \overset{\text{VI}}{\text{R}} : \overset{\text{II}}{\text{R}} = 23.40 : 18 : 6.45, \text{ or nearly } 4 : 3 : 1.$$

The deviation from the simple ratio will not appear so great as seems at first sight, if it be noticed that a difference of one half per cent. in the amount of water would make the ratio almost exact.

The corresponding ratio in the Damourite from Wermland is

$$23.15 : 17.78 : 6.41, \text{ or, as before, nearly } 4 : 3 : 1.$$

These ratios point to the general formula



The Damourite and Sterlingite are types of a very large class of hydrous micas, which in many places are widely distributed through the granitic rocks. This class of minerals has been especially investigated by Professor Haughton, of Dublin, whose papers may be found in *Phil. Mag.* S. 4. vol. ix. p. 272, and *Quart. Journ. Geol. Soc.* vol. xviii. p. 403, also vol. xx. p. 268. We cite here a few of his analyses, selected from those given by Professor Dana (pages 310 and 311 of the fifth edition of his 'System of Mineralogy') in further illustration of the subject we are discussing:—

Dublin Co.—Haughton. Optical angle, 53°.

Si.	Al.	Fe.	Mg.	Ca.	Na.	K.	H.
43·47	31·42	4·79	1·13	1·38	1·44	10·71	5·43 = 99·77
23·18	14·64	1·44	0·45	0·39	0·37	1·82	4·83
23·18	16·08		3·03				4·83
23·18	16·08		7·86				
3	2		1				

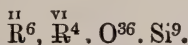
Mount Leinster—Haughton. Optical angle, 72°.

Si.	Al.	Fe.	Mg.			K.	H.
44·64	30·18	6·35	0·72			12·40	5·32 = 99·61
23·81	14·07	1·90	0·29			2·11	4·73
23·81	15·97		2·4				4·73
23·81	15·97		7·13				
3	2		1				

Glendalough—Haughton. Optical angle, 70°.

Si.	Al.	Fe.	Mg.	Ca.	Na.	K.	H.
44·71	31·13	4·69	0·90	1·09	1·27	9·91	6·22 = 99·92
23·84	14·51	1·41	0·36	0·31	0·33	1·68	5·53
23·84	15·92		2·68				5·53
23·84	15·92		8·21				
3	2		1				

In these micas again, if we regard the water as basic, we obtain a nearly constant ratio, but differing from that of Damourite in the relation of the two basic radicals. The general symbol of the last would be



There appear to be, therefore, these two distinct types of hydrous micas related to the species Muscovite, all rich in alumina and alkali, destitute, or only containing very small amounts, of magnesia, and having a wide optical angle.

The anhydrous Muscovites have not been investigated nearly as fully as the hydrous varieties; and I can find no analyses of any of the beautiful specimens from our American localities. I cite here, for the sake of comparison, an analysis of the Fuchsite,

from the Zillerthal, by Shafhäutl (No. 1), and one of a mica, from Fahlun, by H. Rose (No. 2):—

	Si.	Al.	Fe.	Cr.	Mg.	Ca.	Na.	K.	F.
(1)	47.95	34.45	1.80	3.95	0.71	0.59	0.37	10.75	0.35 = 100.92
	25.57	16.05	.54	1.24	.28	0.17	0.09	1.83	
	25.57	17.83			2.37				
(2)	46.22	34.52	6.04		2.11	8.22	0.98	1.03	
	24.65	16.08	1.81		0.84	1.39	0.87		
	24.65	17.89			3.10				

If now we should add to (1) 6.93 per cent. water, and to (2) 3.42 per cent., we should obtain the ratios:—

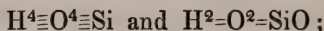
$$(1) \quad 25.57 : 17.83 : 8.52, \text{ or approx. } 3 : 2 : 1,$$

as in the hydrous micas analyzed by Haughton;

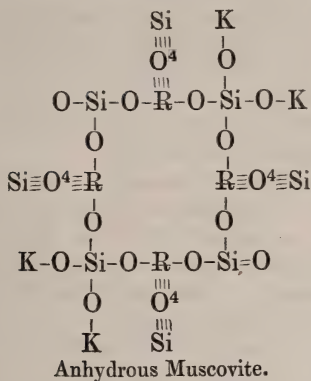
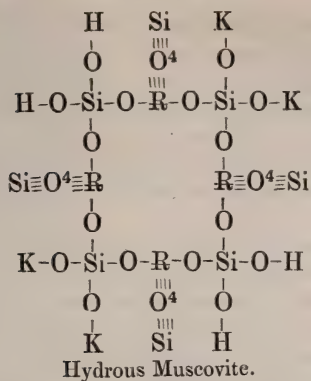
$$(2) \quad 24.65 : 17.89 : 6.16, \text{ or approx. } 4 : 3 : 1,$$

as in Damourite and Sterlingite.

There would seem, then, to be two definite varieties of hydrous micas of the Muscovite family, distinguished by the atomic ratios 3 : 2 : 1 and 4 : 3 : 1. Corresponding to these, it is probable that there are two varieties of anhydrous mica, containing an excess of SiO_2 , which, by the addition of sufficient water to saturate the excess of the acid radical, are reduced to one or the other of the two normal types. In a previous paper (*Amer. Journ. Sci.* (II.) vol. xii. p. 217, 1867) the author suggested the idea that the excess of silica in this class of micas might result from a mixture of two isomorphous species corresponding to the two hydrates,



and he there described a mica whose atomic ratio was very closely that of the second type, and which he called Cryophyllite. Now the simplest theory of the relation of the hydrous to the anhydrous Muscovites would seem to be that, while in the molecules of the anhydrous micas a portion of the silicon atoms (the number varying in the different varieties) are in the condition of the first anhydride ($\text{H}^2=\text{O}^2=\text{SiO}$), the hydrous micas contain sufficient basic hydrogen to bring all the silicon atoms into the condition of the normal hydrate ($\text{H}^4\equiv\text{O}^4\equiv\text{Si}$). The two graphic formulæ which follow indicate more clearly than any other language can, the relations we have attempted to describe. In these formulæ R stands for the double atom in the radical of the sesquioxides, having the quantivalence of six:—



The new mineral *Sterlingite*, whose examination has been the occasion of this discussion, is remarkable as being a very well-defined example of a hydrous mica occurring in large crystals, and exhibiting very marked characters. It does not materially differ in composition from *Damourite*; and it also agrees with the specimens of this mineral so closely in other physical qualities that we cannot regard the small optical angle, observed by Des Cloizeaux on the minute scales of the Pontivy mica, as sufficient ground for separating the new mineral from the old species. We should include under this species all hydrous micas which are rendered by the basic hydrogen orthosilicates; and to it the name *Damourite* belongs by priority. *Sterlingite* is simply a variety of *Damourite*, having the ratio 1 : 3 : 4, with more marked qualities and a wider optical angle than the Pontivy mineral; and, provisionally, the name I have given will be useful in designating it.

The hydrous micas, of which *Sterlingite* is a variety, have a special interest in connexion with the subject of this paper, be-

cause they illustrate the characteristics of basic water, which will be contrasted with those of water of crystallization in our description of the following species. The evidences that the water in these micas is basic (that is, forms a part of the basic radical) may be summed up as follows:—

1. The amount of water in the different varieties is very variable, and bears no constant ratio to that of the other basic radicals*.

2. The hydrogen of the water supplements the other basic radicals and fills out the amount required for a unisilicate, the type to which most of the micas conform.

3. The water is expelled only at a very high temperature.

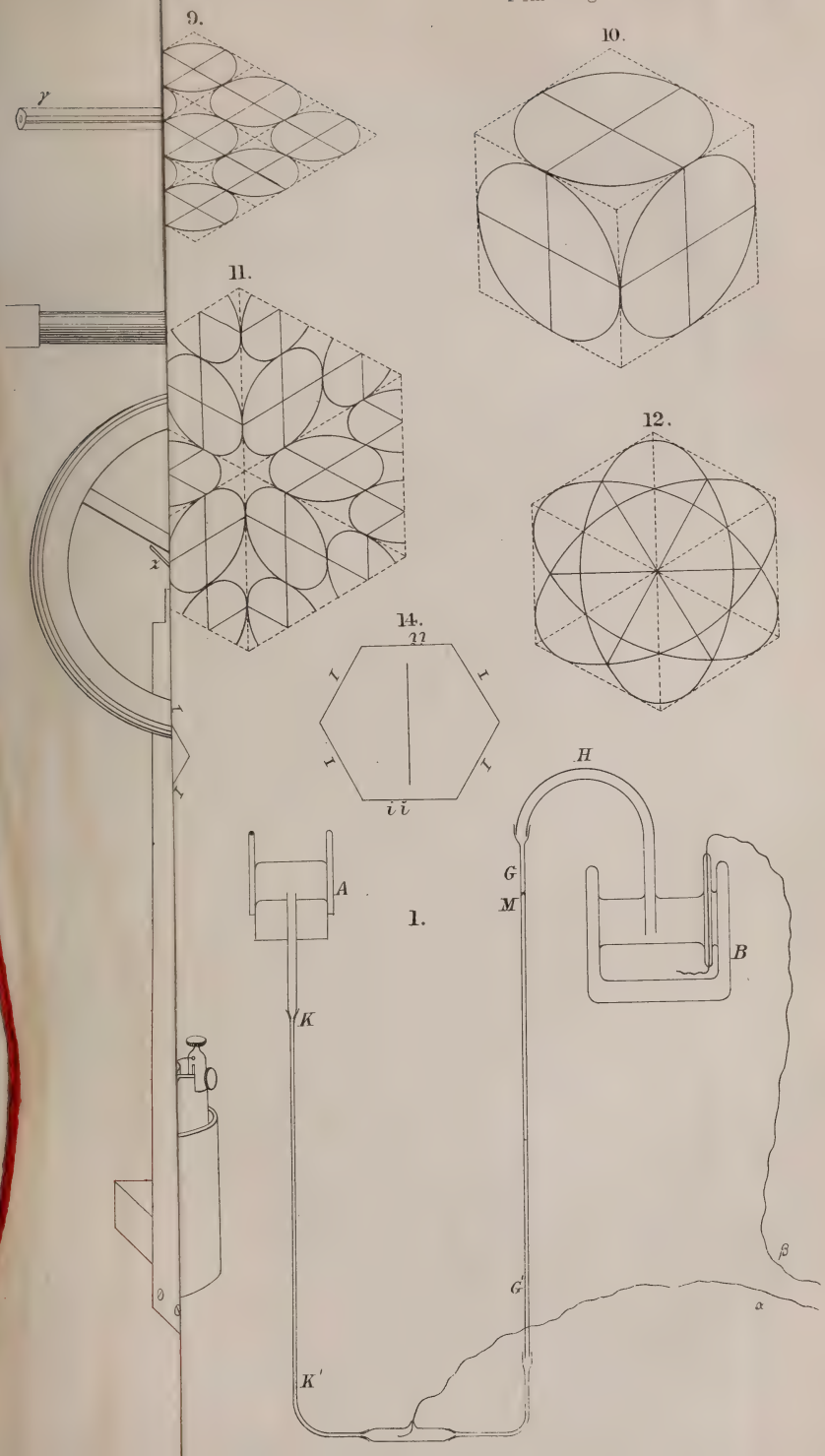
4. The loss of water is not attended with any marked change in the appearance of the mineral†.

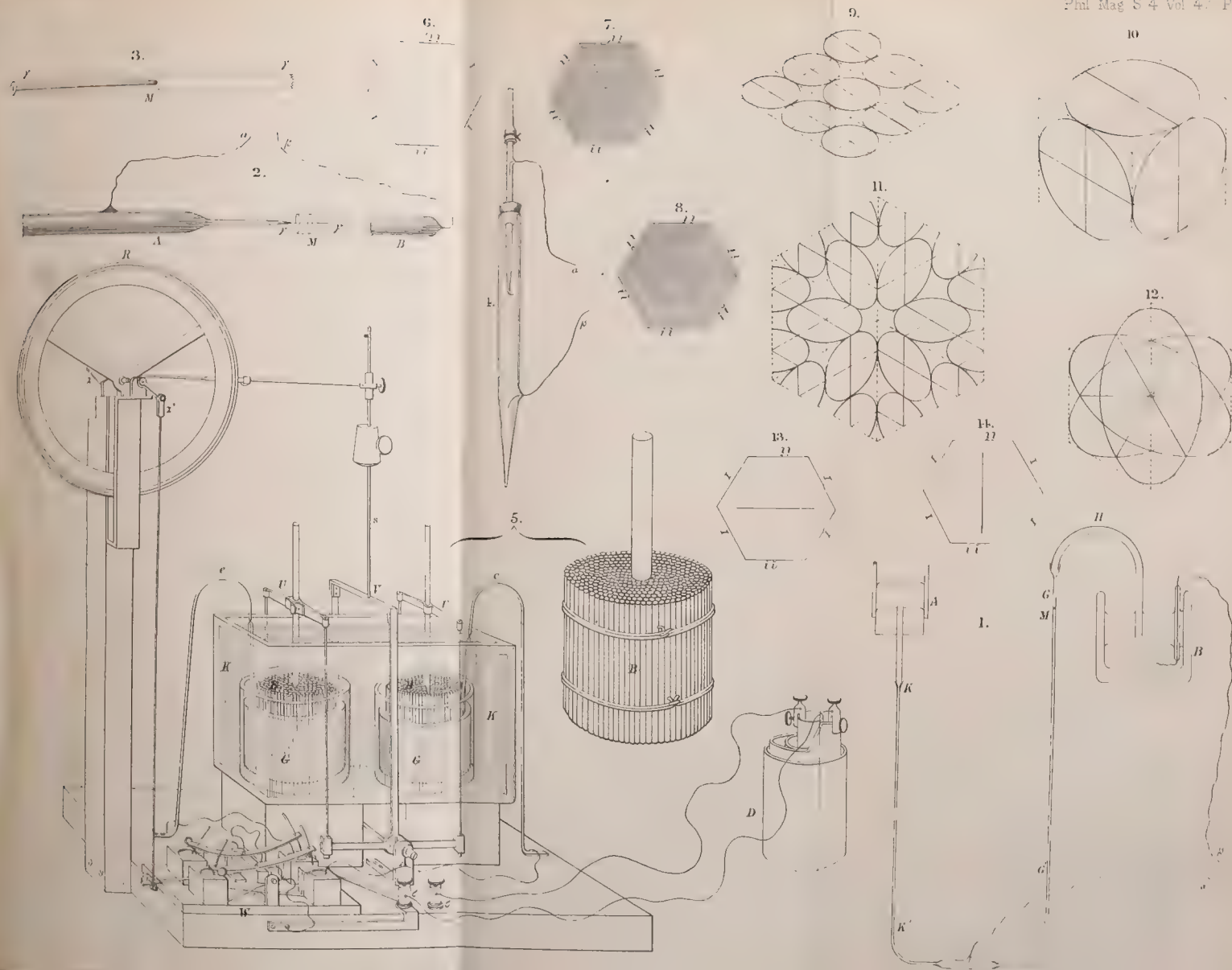
Jefferisite, of *West Chester*.—This well-known mineral, found in the serpentine at West Chester, Pa., was, as I have said, carefully analyzed by Professor Brush, of Newhaven, who named it after W. W. Jefferis, Esq., of West Chester; and to this gentleman I am indebted for the specimens of the mineral whose crystallographic relations I have studied.

The crystals of *Jefferisite* cleave like mica, affording thin but unelastic laminae. The cleavage-planes are marked triangularly by lines crossing at angles of 60° and 120° . In some cases there is a jointing as in crystals of mica, parallel to the shorter diagonal of the rhomb. One crystal sent me by Mr. Jefferis is the half of a rough hexagonal prism $1\frac{1}{4}$ inch high by 2 inches in diameter. The plane of the optical axes, as in the larger number of micas, is parallel to one of these lines, as indicated in Plate III. fig. 6, coinciding with the shorter diagonal of the rhombic prism, which appears to be the fundamental form in all this class of minerals, and from which the hexagonal form is derived by the truncation of the two acute angles. The double refraction is strongly negative; but the angle between the optical axes varies in the most remarkable manner. I have measured angles on different plates, of 27° , 24° , and 10° , and observed many intermediate conditions. Owing to the deep yellow colour,

* This fact is not shown so forcibly by the analysis cited above as by the series given by Professor Dana, on pages 310 and 311 of the fifth edition of his 'System of Mineralogy,' to which we would refer in illustration of this point.

† Since the above was written, we have received from Dr. F. A. Genth his very valuable paper on corundum and its associated minerals. He regards *Damourite* as one of the most important products of the alteration of corundum, and gives a large number of analyses of specimens from different localities, to which we gladly refer, as they illustrate the point made in this paper, even more markedly than the analyses cited above. The *Damourites* are evidently widely distributed minerals and characteristic features of certain rocks.





the plates become opaque at a very moderate thickness; and for this reason it is impossible to measure the angle with great precision. Some of the plates are apparently uniaxial; but this may result from the blending of the two hyperbolas, due to the thinness of the plate. The dispersion of the axes is but slight, and only perceptible in the thicker laminæ when $\rho < v$. It is obvious, therefore, that the crystallographic characters of the mineral are identical with those of mica.

The plates are generally, if not invariably, twinned; and the twinning is the cause of this most remarkable variation in the optical angle, as will be explained at length in connexion with our description of Culsageeite. On this last mineral the same phenomena are more marked, owing chiefly to the greater transparency of the plates.

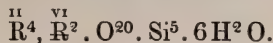
In order to illustrate the chemical relations of the mineral to the Biotite micas, we give below:—(1) the results of the analysis of Jefferisite by Professor Brush; (2) the same results, calculated for the anhydrous mineral; (3) the results of an analysis of a Biotite mica, from Pargas, Finland, by Svanberg. In each case I have added the amounts of oxygen in the several oxides to show the atomic ratios:—

	Si.	Al.	Fe.	Fe.	Mg.	Ca.	K.	H.	
(1)	37.10	17.57	10.54	1.26	19.65	0.56	0.43	13.76	=100.87
	19.78	8.18	3.16	.28	7.86	.16	.07	12.23	
	19.78	11.34		8.37				12.23	
	5	3		2				3	

	Si.	Al.	Fe.	Fe.	Mg.	Ca.	K.	
(2)	42.94	20.33	12.20	1.46	22.75	.66	.50	=100.84
	22.90	9.47	3.66	.32	9.10	.19	.08	
	22.90	13.13		9.69				
	5	3		2				

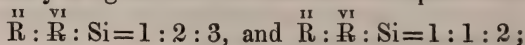
	Si.	Al.	Fe.	Mn.	Mg.	Ca.	K.	H.	F.
(3)	42.58	21.68	10.39	.75	10.27	1.04	8.45	3.35	.51 = 99.02
	22.71	10.10	3.12	.17	4.11	.30	1.43	2.98	
	22.71	13.22		8.99					
	5	3		2					

The general symbol of Jefferisite deduced from (1) would be



A comparison of the results given in (2) and (3) will show that the anhydrous Jefferisite corresponds very closely in its chemical constitution with the Biotite mica from Pargas. The chief difference is to be found in the fact that the mica contains

potassium and basic hydrogen in place of more than one half of the magnesium of the Jefferisite. It should, however, be remembered in this connexion that the Biotites present a very wide variation in the ratio between the amounts of the protoxide and sesquioxide radicals which the various varieties contain. The limits usually assigned to this variation correspond to the ratios



and the Pargas mica, with the ratio 2 : 3 : 5, falls between these limits; but the Culsagee variety of vermiculite corresponds to the more common class of Biotites, which have the ratio 1 : 1 : 2.

But this resemblance in chemical constitution only appears when we compare the Biotite mica with the anhydrous Jefferisite; while it is the crystallized hydrous Jefferisite which so closely resembles the magnesian micas in its crystallographic relations; and the question now arises, What is the condition of the large amount of water ($12\frac{1}{4}$ per cent.) which the crystallized mineral contains?

To aid us in forming a conclusion on this point, we have the following evidence:—

First. As the above analysis shows, the water is united in definite and atomic proportions amounting to six molecules to every five molecules of silicon in the molecules of the mineral—that is, sufficient to convert all the silicon into a hydrate, assuming that the five silicon atoms in this hydrate are joined to each other by the smallest possible number of bonds.

Secondly. While both the crystallographic and the chemical relations of Jefferisite to the other vermiculites, and to the magnesian micas, indicate that the mineral is an orthosilicate, the amount of basic radical, exclusive of the water, is amply sufficient to saturate the atomicity of the silicon.

Thirdly. It was observed by Professor Brush (and his observations have been fully confirmed by ourselves) that the water is given off at a comparatively low temperature—about 300° C.; and, as every mineralogist knows, this dehydration is attended with that remarkable exfoliation which is characteristic of the vermiculites, and indicates a complete disintegration of the molecular structure. This exfoliation is wholly different from the phenomena which the so-called hydrous micas present under like conditions. In these last (which, as we suppose, contain hydrogen as a part of the basic radical of their molecules) a very high temperature is required to expel the water, and the loss is attended by no such marked change of volume and disintegration.

The conclusion that we draw from these facts is that the combined water is in the same condition in Jefferisite as in the zeolites and in many crystallized salts—a condition which has

long been known in chemistry as *water of crystallization*. We shall not here attempt to discuss what are the relations of the water thus combined to the molecular structure of the mineral. This question is still in suspense, and we are persuaded that our science is not yet in a condition to solve the problem. All that we can at present do is to classify the phenomena presented by the exfoliation of the vermiculite minerals with the efflorescence of Glauber's salt or the intumescence of alum.

If, then, I am justified in the inferences I have drawn, Jefferisite differs chemically from Biotite mica chiefly in containing water of crystallization, very much in the same way that gypsum differs from anhydrite, or common salt from the crystals with two molecules of water which form in brine at a low temperature. In these bodies, however, the forms of the crystals of the hydrous are wholly different from those of the anhydrous salts, while the crystallographic relations of Jefferisite are very similar, if not identical, with those of the magnesian micas. It is, however, also true that the crystalline structure of the micas seems to admit of a very wide variation of chemical composition. In cryophyllite, which has all the crystallographic characters of the micas strongly developed, the atomic ratio is 3 : 4 : 14; while in the Biotite, whose analysis was cited above, it is 2 : 3 : 5. The first is an acid, and the last a neutral silicate. The other varieties of mica have a composition intermediate between these extremes; and it might be said, if such a variation of composition is compatible with the crystalline structure exhibited by this group of minerals, it would not be surprising if the structure were sufficiently elastic to admit of the insertion of the water of crystallization without great alteration of external form.

It may be further urged that the normal mica crystal (a rhombic prism with angles of 60° and 120°) is a form which appears to be related to a mode of molecular structure common to a great many substances; and the phenomena which will be described in this paper seem to sustain this opinion. Still we do not care to theorize about the subject. Our object is simply to make prominent the two points:—(1) that the crystallographic structure of Jefferisite is identical with that of the magnesian micas; (2) that the chemical constitution of the anhydrous mineral is closely allied to that of Biotite.

Culsageeite, or the Vermiculite of the Jenks Mine, North Carolina.—Through the kindness of Colonel C. W. Jenks, the proprietor of the corundum mines on the Culsagee river in Macon County, N. C., I have had an opportunity of examining a variety of vermiculite which at that locality forms an important part of the matrix of the corundum. The associations of the mineral in North Carolina are very similar to those in Pennsylvania.

Professor J. Lawrence Smith, in his recent article in *Amer. Journ. Sci.* (III.) vol. vi. p. 180, says that "in the development in North Carolina the corundum" (with which the vermiculite is associated) "occurs in chrysolite or serpentine rocks, and outside of serpentine it has not been found. These chrysolite rocks belong to a regular system of dikes which have been traversed for the distance of about one hundred and ninety miles. This system of dikes lies on the north-west side of the Blue Ridge, and has a strike parallel to the main mass of the ridge, and has an average distance from the summit of the ridge of about ten miles. The serpentine appears at intervals along this whole line of one hundred and ninety miles," and "is usually enclosed in a hard crystalline gneiss." In the serpentine the corundum has been found at several localities along a distance of forty miles. Colonel Jenks writes me:—"The workmen have opened a new vein they call the gem vein. It is walled with chrysolite; and the fissure is from one to four feet wide, which fissure is filled with ripidolite; and in this the corundum crystals are imbedded." As regards the association of the ripidolite and vermiculite, he also writes:—"They occur both alone and intermingled, and also in layers, like the leaves of a book." Again, "The ripidolite maintains a uniform character of texture and appearance, whether associated with the vermiculite or alone. In the vein at the bottom of the shaft I have spoken of, and elsewhere, it is of uniform hardness, texture, &c.; but in it the corundum seems to have lost cohesion—in some localities falling to pieces, and crumbling out of the ripidolite matrix. But when exposed to the air and sunshine for a day or two, the corundum regains its cohesion, and you can then chip off the ripidolite from it. It is just the opposite with the vermiculite when alone the matrix. Sometimes it occurs in masses several feet long, formed of scales as large as your hand, easily separating like mica from the corundum, which in this matrix always seems to retain its cohesion. Then, again, the vermiculite will occur in masses of many bushels, all broken up and disintegrated, like very coarse bran, in which the crystals of corundum lie like eggs in a box of sawdust. The great crystal (weighing 316 pounds) was thus deposited. At other places the entire mass seems to have changed to the colour of white Castile soap, with specks of green in it, the corundum crystals all falling apart until exposed, when they harden." This untechnical language of an unprofessional but accurate observer conveys a very vivid idea of the mode of association of these minerals. The specimens so kindly sent me by Colonel Jenks confirmed in all respects his observations. In several cases the corundum was still imbedded in both the ripidolite and the vermiculite; and the scales of these minerals

not unfrequently penetrated the corundum crystals in such a way as to indicate that the corundum had crystallized around them. The ripidolite is firm in texture, and closely resembles the variety from West Chester. It occurs in hexagonal plates, which have a striation similar to that I formerly described as characteristic of the Pennsylvania specimens (*Amer. Journ. Sci.* (II.) vol. xlv. p. 201, 1867), and which is illustrated by fig. 7. Professor Smith (*loc. cit.*) gives analyses of two varieties of ripidolite (one compact, the other friable) as follows:—

Compact Ripidolite from North Carolina.

Si.	Al.	Fe.	Mg.	H.
27·00	21·60	16·63	22·00	12·30 = 99·53.

Friable Ripidolite from North Carolina.

Si.	Al.	Fe.	Mg.	H.
29·15	10·50	23·50	25·44	10·04 = 98·63.

A determination of the silica in the compact mineral, made by Mr. F. Gooch, student in the laboratory of Harvard College, gave 27·25 per cent. of SiO_2 , and 11·93 H^2O , as the mean of two closely agreeing determinations. But in the specimens sent me by Colonel Jenks I did not recognize the friable variety.

The vermiculite occurs in close contact with the ripidolite, and is frequently interlaminated with it; *but the two are always perfectly distinct*, thus entirely disproving the theory that the vermiculite was derived from the ripidolite by weathering. It occurs in large plates having more or less of an hexagonal outline. Some of those received from Colonel Jenks were 5 inches in diameter. It has a greenish-yellow colour, which is very much lighter than that of the West-Chester variety. The plates are strongly marked by lines crossing at angles of 60° and 120° , like those from West Chester; but these lines are more marked in the North-Carolina variety. This variety is also much more friable than the other, and readily breaks in directions parallel to these lines, yielding rhombic plates with angles of 120° and 60° , and, more readily, hexagonal or triangular plates, produced by the truncation of the 60° angle of the rhombic plate, on a line parallel to the shorter diagonal of the fundamental rhomb. The plates most readily break parallel to this diagonal, and, like the specimens from West Chester, are frequently jointed in this direction. Like other micaceous minerals, the plates cleave readily parallel to the basal plane, yielding very thin laminæ, exceedingly flexible, but not elastic. The optical characters are the same as those of the West-Chester variety,—strong negative double re-

fraction yielding a biaxial ring system, with uniform distribution of colours, and very variable optical angle. I have measured angles from about 30° to about 13° . The angle often varies widely in different parts of the same plate. Thus I have measured on different laminæ, from a single plate not exceeding 3 inches in diameter, the three angles 30° , 24° , and 13° ; and, again, I have noticed a similar variation on one and the same lamina. Indeed the phenomena which I observed were almost identical with those I had previously observed on plates of ripidolite from Texas, Pennsylvania (*loc. cit.*). On moving the lamina just referred to parallel to itself, in front of the polarizing microscope, the optical angle varied as I passed from one side of the field to the other. Beginning with a value of about 30° , the angle decreased to about 13° . Moving the plate still further, I found a region of indistinctness; and then the axes opened again, the new plane making an angle of 120° with the old. I had evidently here a macling precisely similar to that I had previously described in the Texas ripidolite, and shown in fig. 8, where the lines of shading represent the position of the plane of the optical axes. This represents what we may call an ideal macle; for I have seldom been able to trace more than three individuals on the same plate, and, as a rule, these are very unequally developed. On many of the specimens of the North-Carolina vermiculite, the macling is externally marked by the eminent cleavage or jointing parallel to the shorter diagonal of the rhomb section; and in several of the specimens I have examined it was quite symmetrical. A study of these specimens led me to an explanation of the cause of the remarkable variation of the optical angle, which I believe not only applies to the vermiculites and ripidolites, but also, in many cases at least, to the micas. It would be expected that the several members of such macles as fig. 8 represents would penetrate each other; and I therefore made a series of experiments to ascertain what would be the effect of the interfoliation of laminæ in which the planes of the two sets of optical axes had the same relative position as in the several members of the macle. To that end I divided a plate, which presented the largest optical angle I had observed, into as thin laminæ as possible, and then superimposed them in the relative position I have mentioned, so that the planes of the optical axes should be inclined at an angle of 60° . The result was (when the thickness of the plates in each position was nearly equal) that a symmetrical ring system was obtained in which the optical angle was about 13° —the smallest I had measured; and by varying the relative thickness, intermediate degrees of optical divergence were produced. By now introducing laminæ into the compound crystal in the position of the third member of the

macle (that is, with the plane of the optical axes in the position of the third diagonal of the hexagon), the apparent angle could be reduced still further, so that the plate was apparently uniaxial. Although these experiments were sufficient to show that the mactling was an adequate explanation of the apparent variation of optical angle I had observed in the plates of ripidolite and Jefferisite, they also raised the question how far the effect I had obtained in my experiments might be due to the circumstance that, on account of the deep colour of these minerals, it is only possible to experiment on very thin plates, with which, of course, the rings of interference are very wide, and the hyperbolas proportionally indefinite. I therefore next made a similar experiment with a well-known phlogopite mica from Jefferson County, N. Y., whose crystals are very distinctly macted after the type of figs. 8 or 10, the plates presenting a variation of optical angle similar to that I have described, the normal axial divergence being about $15\frac{1}{2}^{\circ}$. A very clear portion of one of these plates was first cut into a regular hexagon, one of whose diagonals was in the plane of the optical axes. This hexagonal plate was then split into twelve laminæ, which were superimposed with the intervention of balsam, and in alternating positions, like the members of a macle,—the optical plane in each of the laminæ making an angle of 60° with that of the lamina above or beneath it. The result was an essentially uniaxial plate, differing from a plate of uniaxial mica only in small irregularities in the contour of the rings, such as the lamination would be expected to produce. On repeating now this experiment with a Muscovite mica having a wide optical angle about 63° , I obtained a most remarkable and unexpected result—a structure presenting optical phenomena similar to those of a plate of quartz cut perpendicularly to the principal axis. At the first trial I obtained a compound mica plate showing a disk of colour in the centre of the field whose tint changed during the rotation of the analyzer (of the polarizing microscope), like a plate of left-handed quartz; and on superposing a plate of right-handed quartz the spirals of Airy at once appeared. The rings, however, were wholly broken up, and appeared only in irregular patches of colour. This irregularity was due to the unequal thickness of the laminæ employed, and I found it impossible to split up one and the same hexagonal plate of mica into laminæ which were sufficiently uniform for the purpose. But very satisfactory results were obtained in the following way. I selected for the purpose the very clear and easily cleavable mica from Grafton, N. H., and, after a few trials, succeeded in cleaving off very thin plates of considerable size and nearly uniform thickness. Selecting one of these plates, I first divided it by means

of a parallel ruler into strips, and from these strips cut out the hexagonal laminæ by means of a steel pattern carefully made. The thin mica can be cut with perfect accuracy by a sharp knife on a plate of glass. The hexagonal laminæ thus obtained, though coming from different parts of the mica plate, were optically parallel to each other; and by drawing at the outset a line with a sharp point near the corresponding edges of the several strips, this line served as a guide for placing the hexagonal laminæ. From laminæ thus prepared, plates were made showing the familiar system of rings as perfectly as could be expected. The best results were obtained with plates consisting of from twelve to twenty-four laminæ; and the character of the resulting plate, whether left- or right-handed, was found to depend on the order of the spiral arrangement. If in building up the pile the marked side of each successive lamina is turned through an angle of 60° in the direction of the motion of the hands of a watch, the result corresponds to left-handed quartz if turned in the reverse direction to right handed; and on superposing two dissimilar plates thus prepared I obtained again the spirals of Airy in great perfection. Thus, then, it appears that, even with micas of the widest optical angle, we can build up a structure which is optically uniaxial*.

* The great difficulty in preparing these plates is to obtain thin films of mica of uniform thickness which are of sufficient size to yield a dozen or more laminæ; and the more nearly we have succeeded in preparing such a film by splitting sheets of mica, the more closely we have been able to imitate the phenomena seen under like conditions with a plate of quartz. We have been able to work with films which measured with a spherometer only $\frac{1}{400}$ of an inch in thickness, and have not obtained good results with those which were much thicker; and when thinner than this the mica cannot readily be cut into shape. The least inequality in the thickness of the several laminæ composing the same plate more or less mars the effect; and although some of the striking features seen with quartz may remain, such as the succession of colours on revolving the analyzer, and even the spirals of Airy, yet the more delicate phases of the phenomena disappear. The plate changes colour when revolved in its own plane, the rings lose their circular form and become confused, and the violet cross disappears. Moreover, as regards the conditions which determine the phase of the circular polarization, the law stated above can only be affirmed with certainty of plates consisting of laminæ which very nearly fulfil the conditions we have described. Very small inequalities of thickness renders the effect irregular, and made it at first difficult to discover the law. Our experiments have been a series of approximations; and although we may never be able with our rude appliances to compete with nature in the manufacture of uniaxial crystals, yet we have approached so near to the perfect result as to be able to point out with confidence one way at least by which the effects seen in natural crystals may be produced. We have usually cut the laminæ into regular hexagons; but equilateral triangles might more easily be cut, and would probably give as good results; for although the errors of position might not be so well distributed, we have found that a slight variation in

The theory which I have formed to account for these facts is as follows. We may conceive that the molecule of mica is an ellipsoid, whose elliptical section, through the longer axis, can be inscribed in the rhomb of 60° and 120° . Assume now that these molecules have polarity, the rhombic prism would be the normal result of their association, as represented in fig. 9. We may, however, conceive that three of these molecules may become associated by alternate single poles to form a nucleus, such as represented in fig. 10. Such a group once formed would be in a condition of great stability, resulting from the concurrent action of the several poles; and if now each of the molecules developed into a crystal, the result would be a macle of a form which is very common in the mineral kingdom. If six molecules united in a similar way to form a nucleus, as in fig. 11, we should also have stable equilibrium (although less firm than before), and the result of a symmetrical development would be a macle such as has been represented in figs. 7 and 8. Whether this more complex arrangement is necessary in order to explain the phenomena presented by the vermiculites and micas I do not feel confident. It is seldom that more than three individuals can be distinguished on a given plate; and the very unequal development of the several individuals and the indefiniteness of the lines of demarcation, resulting from the phenomena which have been described, render what would seem to be a characteristic feature of the more complex group not necessarily a certain indication of the structure. I refer to the fact very constantly noticed, that the plane of the optical axes is parallel to the nearest hexagonal edge, as shown in figs. 7 and 8. In figs. 10 and 11 this same plane is parallel to the shorter axis of the ellipse; and it can easily be seen that, if either of the individuals of fig. 10 were developed over any large portion of the space of its neighbours, the optical plane might appear parallel to the adjacent edge.

Having made the two suppositions as above to explain the phenomena of twinning, which have been long familiar and externally visible, it will not, we trust, appear unreasonable if we make a third supposition to explain the phenomena first described in this paper. We may conceive that, the ellipsoidal molecules, instead of grouping together on the same plane, become associated by their alternate poles, one over the other, as represented in fig. 12. Molecules so associated, developing laterally, would

the relative position of the laminae injures the result to a far less degree than the least inequality in their thickness. It still remains to make careful quantitative measurements of the effects produced under determinate conditions; and, as can easily be seen, the subject opens a wide field for mathematical analysis as well as physical investigation.

produce the laminæ of a mica plate in the relative position in which we have placed them in our artificial crystals, with only this difference, that the laminæ would be indefinitely thin and in exact position; and the effect of such compound molecules in modifying the elasticity of the crystalline structure must be, in most respects at least, like that of single molecules symmetrical on all sides of one line or axis; in other words, they must produce a structure similar to that of uniaxial crystals. Under what further conditions the grouping of the molecules, in right- or left-handed spirals, determines the phenomena of right- or left-handed circular polarization, and what bearing the new facts may have on the received theory of these phenomena as they appear in quartz, must be left for further analysis to discuss.

I pass next to consider the composition of the Culsagee vermiculite; and I give below at (1), (2), and (3), the results of three analyses made by myself, together with the corresponding oxygen-ratios.

The pulverized mineral, after it has been exfoliated by heat, is easily and perfectly decomposed by hydrochloric acid. In analysis (1), after the separation of the silica, the alumina and ferric oxide were separated from the magnesia by ammonia with the usual precautions. In (2) and (3) the bases were converted into nitrates and separated by Deville's method. In each case the magnesia was weighed as pyrophosphate, and the alumina and ferric oxide were weighed together.

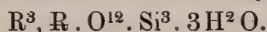
All three analyses were made with material rendered anhydrous by ignition until the weight was constant; and each is represented by three distinct determinations—namely, the weight of the silica, the sum of the weight of the alumina and ferric oxide, and the weight of the magnesian pyrophosphate. The oxides of iron and the water were once for all determined on separate portions of the dried but not exfoliated mineral, and therefore appear of the same value in all the analyses.

The determination of the water was the only difficulty which the analysis of this mineral presented. It is by far the most hygroscopic silicate I have ever examined, when once dry absorbing water from comparatively dry air with almost as much avidity as chloride of calcium. In two experiments with different portions of the same powder, the material was heated in an air-bath at 100° C. for seventy-two hours before the weight became constant; and in each case the weight was compared at intervals of about six hours. The total loss in the first experiment was 10·27 per cent., and in the second 10·19 per cent. The mineral thus dried lost, when ignited, 10·84 per cent. Another portion of the same powder, which had been dried over sulphuric acid for more than two months, lost, when ignited, 11·09 per cent.

This close agreement indicates that all the water lost in drying, either at 100° or over sulphuric acid, is hygroscopic; and the conclusion is very strongly confirmed by the fact that the mineral in thus drying does not change its aspect in the least degree, and rapidly reabsorbs the water when exposed to the air. On the other hand, when the mineral is ignited it swells up to many times its volume, like other members of the vermiculite family, and undergoes what is evidently a profound alteration in its molecular structure.

	Si.	Al.	Fe.	Fe.	Mg.	H.
(1)	37.58	19.73	5.95	0.58	25.13	11.09 = 100.06
	20.04	9.19	1.78	0.14	10.05	9.86
	20.04	10.97		10.19		9.86
	2.03	1.11		1.03		1
(2)	37.43	19.75	5.95	0.58	25.58	11.09 = 100.38
	19.96	9.20	1.78	0.14	10.23	9.86
	19.96	10.98		10.37		9.86
	2.03	1.11		1.05		1
(3)	37.10	20.22	5.95	0.58	25.07	11.09 = 100.01
	19.79	9.42	1.78	0.14	10.03	9.86
	19.79	11.20		10.17		9.86
	2	1.13		1.03		1

It is evident from these analyses that the atomic ratio of the mineral is 2 : 1 : 1 : 1; and its formula may therefore be written



By referring to what has already been said of the relation of Jefferisite to the Biotites, it will be seen that while that mineral corresponds to a less common variety of this species, the Culsagee vermiculite corresponds to its more usual type. We give below at (1) the results of an analysis of Vesuvian Biotite by Chodnef, and at (2) the same, assuming that a portion of the iron is in the ferrous condition, as is well known to be the case, in order to show that values within the probable error of the analysis would give the ratio 2 : 1 : 1. Here, of course, the alkali takes the place of a portion of the magnesia of the vermiculite.

	Si.	Al.	Fe.	Fe.	Mg.	Ca.	K.
(1)	40.91	17.19	11.03	..	19.04	0.30	9.96 = 98.43
(2)	40.91	17.19	7.03	4.00	19.04	0.30	9.96 = 98.43
	21.82	8.30	2.11	.89	7.62	0.09	1.69
	21.82	10.41		10.29			
	2.12	1.01		1			

This new variety of vermiculite is so well marked, and the composition so definite, that I have thought best to designate it by the name *Culsageeite*. As regards its other characters, it has a specific gravity of 2.225 (taken in alcohol), and about the hardness of talc. Before the blowpipe the exfoliated mineral fuses readily to a white enamel, but does not fuse in the flame of a Bunsen lamp.

To this variety of vermiculite belongs, as I have already intimated, the original mineral from Millbury, analyzed by Crossley. There can be no question as to the general accuracy of Crossley's results; and assuming that all the iron is in the ferric condition, they give, as I have shown, almost precisely the atomic ratio 2 : 1 : 1 : 1. The only question that can arise is in regard to the condition of the iron. I have therefore made an assay of the iron by the accurate method I formerly described in the *Amer. Journ. Sci.* vol. xlv. p. 347 (1867). One hundred parts of the massive mineral gave 7.40 per cent. ferric oxide and 3.86 per cent. ferrous oxide, which corresponds to a total of 10.56 ferrous oxide. Crossley found 10.02 ferrous oxide; but he separated with great care the vermiculite from the steatite with which it is mixed; and this steatite was probably the source of the greater part of the small quantity of ferrous oxide found in our assay, which, though not made with pure material, shows conclusively that the condition of the iron in the Millbury vermiculite is not different from that in other varieties of the same family of minerals.

Hallite.—Several years since, this variety of vermiculite was sent to me by Mr. John Hall, of Philadelphia, by whom it was originally discovered. The examination then made showing that the mineral was a new variety, if not a new species, of the vermiculite family, I gave to it the name of *Hallite*, in recognition of the mineralogical services of Mr. Hall, who not only discovered the mineral, but has carefully worked the locality and observed the associations in which it is there found. A preliminary notice of the mineral under this name was published at the time by Professor Leeds, of Hoboken; but the interesting relations which the mineral bears to the subject of this paper have made a further examination desirable.

Hallite occurs in large rough six-sided prisms, with easy micaceous cleavage. There are two varieties, differing markedly in colour—green and yellow; and I am indebted to Mr. Hall for the following facts in regard to the locality and associations of this species. Mr. Hall writes:—"The mineral is found at East Nottingham, in the serpentine formation of south-eastern Pennsylvania, three miles south of Oxford, in Chester County; and I know of no other locality. I think the green and yellow varieties are very closely related, and may possibly pass from one into the other; but I have no positive proof that they do. The crystals are found in nests or pockets; and the two colours are not found in the same nests. The green crystals are imbedded in a steatite earth or base of the same colour as the crystals, and the yellow in a yellow earth; and sometimes nests containing the opposite varieties are only a few feet apart in seams of the serpentine rock."

As the following analyses show, the two varieties have essentially the same composition; and the only difference that could be detected was in the degree of oxidation of the iron. The yellow crystals appeared to be more weathered than the green; and on the last the green colour frequently fades out towards the centre of crystals, thus giving indications of a metamorphosis by which one variety may pass into the other.

Under the microscope these scales of the mineral show a remarkable appearance. Between the greenish or nearly colourless plates are seen elongated scales of a yellow mineral resembling closely in colour thin scales of *Jefferisite*. They are more or less spear-shaped in form, although usually very narrow, and lie accurately in parallel lines, which cross at angles of 60° and 120° , like the magnetic oxide of iron in the *Muscovite* from Pennsbury, Pa., or the microscopic crystals in the *Biotite* of South Burgess of Canada; and the phenomenon of asterism, seen so beautiful with the plates of the last, can also be seen with thin laminæ of Hallite. It was impossible to free the mineral from this admixture; but specimens were selected for analysis as free from it as possible. It was also impossible to determine its exact nature. The scales had not a definite form; but there was a tendency to a rhombic shape, which is well described by the term "spear-shaped;" and though the material is so widely distributed through the crystal, the total mass must be very small.

This mineral is not so hygroscopic as *Jefferisite*, and no difficulty was found in drying the material for analysis. When ignited, it exfoliates like other species of *vermiculite*, but not nearly to so great an extent as *Jefferisite*. After ignition it is decomposed by hydrochloric acid. The specific gravity of the green variety, mean of four determinations, 2.398; that of the

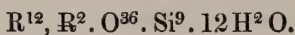
yellow variety, mean of two determinations, 2.402. Before the blowpipe, fuses with difficulty to a brown enamel. The following analyses were made by Mr. C. E. Munroe, assistant in the laboratory of Harvard College:—

Green variety of Hallite.

	Si.	Al.	Fe.	Fe.	Mg.	K.	H.
(1)	35.97	7.61	8.83	1.13	31.34	0.43	14.32 = 99.63
(2)	35.80	7.29	8.73	1.13	31.56	0.49	14.33 = 99.33
Mean.	35.89	7.45	8.78	1.13	31.45	0.46	14.33 = 99.49
	19.14	3.47	2.63	0.25	12.58	0.08	12.74
	19.14	6.10			12.91		12.74
	3	0.96			2.02		2
	3	1			2		2

Yellow variety of Hallite.

	Si.	Al.	Fe.	Fe.	Mg.	K.	H.
(1)	35.17	7.74	9.76	0.32	31.61	0.56	14.65 = 99.81
(2)	35.34	7.42	9.61	0.32	31.41	0.65	14.91 = 99.66
Mean.	35.26	7.58	9.68	0.32	31.51	0.61	14.78 = 99.74*
	18.81	3.53	2.90	0.10	12.60	0.10	13.14
	18.81	6.43			12.80		13.14
	3	1.03			2.04		2.09
	3	1			2		2



It will be seen from the above analyses that, although the atomic ratio between all the basic radicals and the silicon is the same as in Culsageeite, Jefferisite, and Biotite, the ratio between the protoxide and sesquioxide radicals is very different. In this respect the mineral resembles the phlogopite micas, in which also the protoxide radicals preponderate; and the symbol given above for Hallite, less the water, is identical with that given by Professor Dana as the more probable formula of the phlogopites.

The opacity produced by the interspersed material made it difficult to determine the optical characters of the mineral, as the rings produced with polarized light could only be seen with very thin plates, and the cross was therefore ill defined; so that, although in some cases there appeared to be a separation of the hyperbolas, the plates could not be distinguished from uniaxial. On one specimen the hexagonal form was very perfect, and the crystal presented the planes of a rhombohedron having an angle

* Trace of manganese.

over the basal edge of about 122° , resembling the crystals of Biotite from Greenwood Furnace. Mr. Hall informs me that these more perfect crystals have only been found in one pocket of the serpentine.

The distinction, however, between the phlogopites and the Biotites is not fundamental, either chemically or physically. Chemically, both species are orthosilicates; that is, the atomic ratio between the silicon and the sum of the basic radicals is 1 : 1. The species differ in composition only in the relative proportion of the sesquioxide and protoxide radicals. In the phlogopite

the ratio of $\overset{\text{II}}{\text{R}}$ to $\overset{\text{VI}}{\text{R}}$ is probably normally 2 : 1; but of the published analyses the value varies between that ratio and the ratio 3 : 2. In the Biotites the same ratio is probably normally 1 : 1; but here, again, the different analyses which have been made give values varying between 5 : 3 and 1 : 2. In like manner the optical distinction between the phlogopites and Biotites, of which so much has been made, is equally indefinite. Between a so-called phlogopite like that from Jefferson County, N. Y., with an angle of about 15° , and the apparently uniaxial plates of Biotite from Vesuvius, there is every possible gradation—sometimes, as I have shown, on one and the same mica plate; and I have endeavoured in this paper to explain the cause of this variation. With the Vesuvian Biotites themselves (if the specimens in the mineralogical cabinet of Harvard College are fair representatives of the mineral from that locality) it is only occasionally that we find a perfectly uniaxial plate. More commonly there are distinct evidences of twinning; and on the borders of the hexagonal plate may be discovered a biaxial structure, of which the optical plane is parallel to different edges of the hexagon on different parts of the plate.

It must, however, be remembered that as, by the process of twinning we have described, the structure of the magnesian micas approaches that of uniaxial crystals, rhombohedral and other planes characteristic of the hexagonal system begin to appear on the crystal. This is illustrated not only by the crystals of Biotite from Vesuvius and from Greenwood Furnace, N. Y., but also by the more perfect crystals of Hallite from Chester County, Pa. In other words, *the process of twinning we have illustrated in this paper produces hexagonal crystals in external form as well as in optical characters*; and the question naturally arises, May not the hexagonal crystals of other minerals be formed in a similar way? that is, may they not be developed from twinned molecules, which, though in their aggregate producing an hexagonal structure, singly would develop into biaxial crystals? Bearing on this point we have discovered some very remarkable evidence.

We have in our possession a plate of Elba tourmaline cut perpendicular to the axis, in which the polarizing microscope shows on different zones a separation of the hyperbolas, which amounts in some positions to 8 degrees; and in moving the plate across the field the optical divergence varies precisely as on plates of phlogopite and vermiculite. There is certainly no external evidence of lamination on tourmaline crystals; for the mineral is remarkably compact, and the crystals have not even a basal cleavage: but it will be remembered how readily some of the varieties pass by alteration into micas of the magnesian type; and this change to a foliated structure, in which the lamination is parallel to the base of the original hexagonal crystal, may be facilitated by a grouping of the molecules of the tourmaline in the manner represented by fig. 12.

We have also a plate of amethystine quartz, in which a beam of parallel polarized rays of light exhibits a twinning almost as symmetrical as that shown in fig. 10, the three zones being most beautifully mapped out by the alternating bands of right- and left-handed quartz, which are such a familiar phenomenon of these crystals. But, besides this, in each of these zones, near the border of the plate, can be distinguished a biaxial structure with an optical divergence of several degrees; and on one other plate of amethyst we have had an opportunity of examining, we have also seen under the polarizing microscope the biaxial curves at one or more points.

These facts most distinctly suggest the theory that the optical phenomena of quartz are produced by a molecular structure similar to that by which we have obtained identical phenomena in our artificial plates of mica, and that the two orders of crystals are aggregates of compound molecules, whose parts are twinned together in the one case in right-handed, and in the other in left-handed spirals, and, lastly, that the simple molecule, if developed normally, would produce a biaxial structure*. This theory is most markedly in harmony with the chemical relations of silica. The compound SiO_2 is the only one of the tetrad

* Since the above was in type we have received the Amer. Journ. Sci. (IV.) February 1874, containing a description of the rhombic silica which Professor Maskelyne, of the British Museum, has discovered in the meteorite of Breitenbach. This new species of silica, which Professor Maskelyne calls Asmanite, has the form of a right rhombic prism, with an angle of $120^\circ 20'$, and the crystals are optically biaxial; but while the specific gravity of quartz is 2.6, that of Asmanite is said to be 2.245. It is perhaps to be expected that such a molecular maeling as we have described would determine an increase of density, since thereby three molecules coalesce to form one; or it is possible that the remarks made *infra* in regard to calcite apply also to quartz; but still the marked difference remains to be explained.

oxides which crystallizes in the hexagonal system; and ever since, by the study of the organic compounds of silicon, the quadrivalent character of the element has been made evident, this fact has been a striking anomaly in our chemical classification. Assume, however, that the molecule SiO^2 would develop normally into a rhombic structure, and that the hexagonal form of quartz is solely a result of molecular twinning, and the anomaly disappears. The molecule SiO^2 may be approximately of the same form as the molecule TiO^2 in Brookite; but having the exact dimensions and polar conditions which favour the mode of molecular twinning, described above and represented by fig. 12, it may always develop into hexagonal shapes.

Are, then, all hexagonal forms thus closely related to the rhombic systems of crystals? And do all molecules of the dimensions and polar conditions illustrated by the figures of this article (that is, those which correspond to the rhomb of 60° and 120°) usually develop into hexagonal forms? May not the whole difference between an hexagonal and a rhombic form arise from a slight difference of dimensions, which determines a molecular macling in the one case, and a normal development of the single molecules in the other? These questions point out most interesting lines of investigation, and will recall to the mineralogist a number of facts bearing upon the subject. Allow me to refer to two of the most striking and most obvious.

On the crystals of chrysoberyl the rhombic angle is $119^\circ 46'$; and every mineralogist is familiar with the hexagonal macling, similar to fig. 7, which is so very characteristic of this species*. Corundum differs chemically from chrysoberyl in that a portion of the alumina in the former is replaced by glucina in the latter. Corundum has a perfect hexagonal form; and, fundamentally, may not the only crystallographic difference be that, in consequence of the replacement, a rhomb of 120° changes to a rhomb of $119^\circ 46'$? Now we have a plate of macled chrysoberyl, showing the normal wide divergence of the optical axes at certain points on its borders, and a nearly uniaxial structure at the centre, where there is an obvious interpenetration between the individuals of the macle, and where the superposition of the several laminæ is most beautifully shown by a polarized beam of parallel rays. We have also a section of a corundum crystal, presenting phenomena similar to those seen with the plate of tourmaline, described above. Further, we have observed like phenomena on a section of phenacite; and although the last mineral contains silica, yet if the molecules of SiO^2 are crystallographically equivalent to those of Al^2O^3 , it may be that the molecular structure

* See also Dana's 'System of Mineralogy,' fifth edition, figs. 154, 155, p. 156.

both of phenacite and of beryl is more closely allied to that of chrysoberyl and corundum than the received theory of their chemical constitution would indicate.

We would not convey the impression that in all these crystals the appearances we have described are strongly marked, or that they have passed wholly unnoticed hitherto. Every one who has become familiar with the optical properties of crystals must have noticed that, with many always regarded as uniaxial, there is not unfrequently in some positions a small separation of the cross into the hyperbolas which are characteristic of biaxial structure. But these irregularities, although long known, have never been satisfactorily explained. They have been hitherto residual features not accounted for by the received theory of crystalline structure, which explains so satisfactorily the general order of the phenomena observed with the polariscope. We have endeavoured in this paper to trace their true significance:—first by showing that the appearances we are discussing are precisely similar to the effects which can be obtained by known means with mica plates; and secondly by observing, on different specimens of various minerals, every intermediate stage between the unmistakable effects of twinning on plates of mica or vermiculite, and the delicate phases of the same phenomena seen with sections of crystals of tourmaline, corundum, or phenacite. One other illustration of our theory.

The rhombic angle of Witherite (native baric carbonate) is $118^{\circ} 30'$; and the all but universal hexagonal mactling of crystals of this species is a well-known fact*. The rhombic angle of Aragonite (the corresponding form of calcic carbonate) is $116^{\circ} 10'$; and the much greater divergence of this angle from 120° determines, as is also known, a style of mactling which is usually quite different from that of Witherite. In the isomeric calcite, however, we have the type of all hexagonal forms. Hitherto the crystalline forms of calcite and Aragonite have been regarded as being as widely separated as possible; and a comparison of these two well-known mineral species has furnished one of the most striking illustrations of dimorphism. But may not, after all, the comparatively small physical differences between these two minerals correspond to a crystallographic difference no greater, fundamentally, than the difference between the rhomb of $116^{\circ} 10'$ and the rhomb of 120° ?

The mactles of chrysoberyl and Witherite are illustrations of a general truth, fully recognized in mineralogy, that all rhombic crystals whose angles approach 120° tend to form hexagonal mactles. The optical phenomena described in this paper certainly suggest the theory that a perfect hexagonal form and structure

* See figures, Dana's 'System of Mineralogy,' p. 697.

may be the result of a more fundamental and molecular mactling, which results when the angle is exactly 120° .

Observations of Sénarmont.—The only previous observations which we have been able to find bearing on the subject of this paper are those of the late eminent mineralogist, H. de Sénarmont, of Paris. In a well-known paper (*Ann. de Chim. et de Phys.* 3rd ser. vol. xxxiii. p. 391), Sénarmont showed that salts which were both geometrically and chemically isomorphous might have very different optical relations—for example, that, while the biaxial crystals of two such salts might have the same bisectrix, the plane of the optical axes in the crystals of one might be perpendicular to the corresponding plane in those of the other. He further proved, by crystallizing together two salts so related, that in the crystals of the isomorphous mixtures thus obtained the optical angle varied with the varying proportions of the constituents between the extreme conditions in the crystals of either salt; and by trial he succeeded in forming from two biaxial salts crystals which, in monochromatic light at least, appeared uniaxial. In a later paper (*Ann. de Chim. et de Phys.* 3rd ser. vol. xxxiv. p. 171) Sénarmont applied the principles which he had thus experimentally verified, to explain the variation of the optical angle of the micas. In this paper he seeks to prove, first, that all micas may be referred to a right rhombic prism with angles of approximately 60° and 120° ; secondly, that while in some micas the plane of the optical axes is parallel to the shorter diagonal of the rhomb, as in fig. 13, in others it is parallel to the longer diagonal, as in fig. 14. Interpreting these facts by the results of his experiments on isomorphous salts, he draws the inference that there are, crystallographically at least, but two species of mica which are geometrically isomorphous but optically distinct, that these are represented by the varieties which have the widest optical angle between the axes in either plane, and that all other varieties with optical angles varying from 0° to 70° in either direction are isomorphous mixtures of the two optically distinct conditions of the mineral.

The observations described in this paper, although they prove that another cause may also determine the variation of optical angle in micaceous minerals, do not necessarily invalidate this beautiful theory of Sénarmont. The variations observed with other minerals, not only on different specimens, but with the same specimen at different temperatures, and which are so beautifully seen with the orthoclase from Wehr in the Eifel, and with crystals of selenite, indicate that such variations may be determined by conditions of molecular structure wholly independent of the mactling here described. We have shown that the mactling

does produce the variation in certain cases; and it must remain for future investigation to assign the limits of the influence which this cause may exert. We would only remark in conclusion that, although in the fifty-seven varieties of mica examined by Senarmont he did not note a single instance in which the position of the plane of the optical axes (with reference to the diagonal of the rhomb) was different on different parts of the same specimen, or even on different specimens from the same locality, he does describe and figure several remarkable macles of Muscovite mica similar to those of vermiculite described above and represented by fig. 10. In one instance the plane of the optical axes is parallel to the shorter, and in the other to the longer diagonal of the rhombic prism; but in both cases it has the same relative position in the several individuals of the macles, the three planes forming with each other angles of 60° . On the plate of another macle a difference of optical angle was observed on different portions of the plate; and this effect was probably similar to that we have been studying in this paper. On another plate from the same crystals, he observed a superposition of the laminæ of the different individuals of the macle; and the following language by which this phenomenon is described, and which is the only reference made to it, shows how closely he came to the results recorded in this paper:—

“Une partie seulement du cristal maclé est commune aux trois lames; mais ici certaines plages (pointillées sur la figure) ont un caractère optique tout particulier; elles ne cessent jamais de développer des couleurs dans la lumière polarisée, quelle que soit d'ailleurs l'orientation de la lame cristallisée. Il est évident que dans ces plages il y a superposition de lames appartenant à des cristaux orientés, les uns comme le petit, les autres comme le grand cristal, de façon que quand les unes ont leurs sections principales dans le plan de polarisation, les sections principales des autres font avec ce plan des angles de 60° degrés.

“Cet exemple prouve donc, non-seulement que les micas peuvent se grouper latéralement sans que leurs clivages cessent d'être parallèles, mais que des feuilletés superposés peuvent même appartenir à des cristaux dont l'orientation diffère de 60° degrés. Un pareil mode de groupement, qui ne trouble ni la régularité de la cristallisation, ni la transparence, semble indiquer que l'arrangement moléculaire de ces prismes rhomboïdaux diffère très-peu de celui qui conviendrait au prisme hexagonal régulier.”

XXXII. *On the Sensibility to Light of Bromide of Silver with respect to the so-called Chemically Inert Colours.* By HERMANN VOGEL*.

WITH regard to certain colours (red, yellow, and green) the photographic action, as we are aware, is either very limited or altogether wanting. This circumstance not only throws difficulties in the way of copying coloured objects (oil paintings), but also with regard to taking portraits, in which coloured clothes and likewise a yellow tint of the complexion, light hair, or rosy cheeks are reproduced in abnormal conditions. What is of a light colour, when of a yellowish tint, comes out bright; and this drawback is only to be got over to a certain extent by a subsequent retouching of the negative.

This abnormal insensibility to colour, as regards photographic plates, is pronounced in the most marked manner with respect to the colours of the spectrum, wherein beyond the violet a powerful action is manifested, which in the visible spectrum (according to the researches hitherto made) does not extend deeper than to the line E in the green. (See Dr. Schultz-Sellack, 'Reports of the German Chemical Society,' 1871, p. 211). Researches recently instituted by me by means of bromide of silver, however, have shown that the sensibility of this preparation not only embraces a considerably wider extent of the spectrum, but even that, by the employment of certain accessories, its sensibility may be carried as far as into the red, or, in other words, to regions where hitherto for photography dark night reigned.

I received recently from England some dry bromide-of-silver plates which Wortley prepares in that country for sale, by a process which is only in part divulged. On exposing these plates to the spectrum, I found to my astonishment that they proved to be more sensitive in the green (that is to say, near the line E) than in the light blue (that is to say, near the line F). Here was an instance of sensibility not in accordance with our previous experience—namely, that a colour the chemical action of which was held to be weak, proved more energetic than that of one which was looked upon as powerful. This circumstance induced me to enter at once upon a more intimate examination of the behaviour of bromide of silver with regard to the colours of the spectrum.

I formed my spectrum by the use of a photographic camera and a Steinheil lens, which I adapted to the battery of prisms of a direct-vision spectroscop. The width of the slit was 0.25

* From Poggendorff's *Annalen*, vol. cl. p. 453. Kindly communicated by W. G. Lettsom.

millim. The solar rays were thrown on to it by means of a Foucault's heliostat, for the use of which I am indebted to the great kindness of my friend Dr. Zenker. The length of the spectrum from D to G was 35 millims.

When desirous of comparing my experiments, I chose the time from 11 till 2, and then operated only when there was a cloudless sky, which it is true is but seldom the case at this period of the year. The time of exposure lasted generally ten minutes. The plates were developed with protosulphate of iron. Even in my earliest experiments I found that the sensibility of the bromide of silver extends considerably further than is stated by Dr. Schultz-Sellack, who obtained therewith an action only from the ultra-violet as far as nearly to the line F in the blue. In my experiments the sensibility extended in all cases beyond the line F, more or less far beyond according to the transparency of the atmosphere. Upon this latter point I purpose entering more at length in a separate communication.

I made a trial of the bromide of silver in two forms:—first, as a so-called *wet plate*, that is to say, when moist from an adhering solution of nitrate of silver derived from the silver-bath wherein the plates were sensitized; secondly, as a *dry plate*, obtained by washing off the solution of silver and then drying the plate. (For further details see 'Reports of the German Chemical Society,' 1873, p. 89.) The behaviour of the two classes of plates was different.

The result was that dry bromide of silver exhibits a more extended sensibility for colours than does the bromide which lies beneath a silver solution; when an *acid* development was employed, the latter manifested sensibility up to the middle point between D and E—that is to say, nearly up to the yellow; the former, however, exhibited sensibility 2 millims. beyond the line D, or up into the orange.

The action of the two plates was moreover very different in a qualitative sense. With moist bromide of silver an extremely energetic action is seen between G and F (in the indigo and blue); near F, however, it declines suddenly, and nothing but a faint indication could be traced as far as the other side of E. Dry plates, on the other hand, exhibited a far less decided action in the blue than the wet plates did; this action, however, declined only gradually, and it extended, as has been remarked above, as far as beyond D.

Hence dry bromide of silver is more sensitive for the rays of lesser refrangibility in the visible solar spectrum; moist bromide of silver is most sensitive for the blue rays of greater refrangibility.

For ordinary photographic plates a solution of silver acts as a

powerful "sensitizer" (see the above work, 1873, p. 88); that is to say, it augments their sensibility in consequence of its fixing chemically the iodine or bromine as it becomes liberated during exposure. That this action occurs chiefly in the blue finds its solution without doubt in this, namely that the blue rays are more eagerly absorbed by the wet film than are the others.

As has been already observed, the sensibility of the dry bromide of silver diminishes gradually from the blue to the red. With bromide-of-silver plates as prepared by me I could see nothing of the phenomenon which I had noticed as occurring in such a marked manner with the English bromide-of-silver plates spoken of above, namely a falling off of the sensibility from the violet to the blue, and an increase thereof from the blue to the green. The explanation above offered as to the action of nitrate of silver upon bromide of silver induced me, however, to conjecture that the English bromide-of-silver plates must contain some substance *that absorbs the green in a greater measure than the blue*. It is not unusual to give dry plates a coating of substances of the most varied kinds, such as gallic acid, caffeine, or morphine, all which bodies fix iodine and bromine and exercise a sensitizing action; occasionally, too, a yellowish colouring-matter is added thereto, with the view of retarding the "chemical" blue light thereby. The optical demeanour of these "preservatives" may be looked upon as a matter not yet in any way understood; neither, indeed, is their favourable influence placed beyond all question.

The plates of Wortley contain nitrate of uranium, gum, gallic acid, and a yellow colouring-matter as a coating. In order to ascertain whether this coating exercised any action, I washed one of these plates with alcohol and water, and obtained in fact by so doing a plate that no longer gave any indication of an augmented sensibility in the green. I now made an attempt to impregnate bromide of silver with a substance that absorbs especially the *yellow* rays and fixes free iodine or bromine, in the hope of thereby heightening the sensibility for yellow. I selected coralline, which Professor Liebermann most kindly placed at my disposal. A solution thereof, when greatly diluted, gives in the spectroscope an absorption-band between D and E; in a more concentrated solution the absorption becomes widened out until it reaches beyond D, while, on the other hand, the blue near F is transmitted to a pretty considerable amount.

I dissolved coralline in alcohol and added some of it to my bromide collodion, so that it became of a strong red colour. Dry bromide of silver plates prepared with this collodion were of a decided red tone. On exposure to the spectrum they bore out my anticipation; that is to say, *the plates proved sensitive*

in the indigo, from there their sensibility decreased till the light blue, became weak at F, then increased again, and in the yellow was found to be almost as efficient as in the indigo. Thus a method was attained for preparing bromide-of-silver plates that are acted upon almost as strongly by a colour hitherto held to be chemically inert, namely yellow, as they are by indigo, which hitherto has been held to be the colour possessing the strongest chemical energy.

After this experiment I was justified in hoping that some other bromide-fixing substance endowed with a powerful absorption of the *red* would in like manner heighten the sensibility of bromide of silver for red. Such a body I met with among the green aniline products. The body in question absorbed in a marked manner the red rays in the middle between D and C; when further concentrated this absorption became extended towards D, while the yellow, green, and blue were transmitted almost unimpaired. In point of fact a collodion coloured with this green proved sensitive to light *as far as into the red*.

The sensibility fell off from the indigo to the yellow, then increased again; and at the identical place where the above-mentioned absorption-band was visible a powerful action in the red was manifested.

From these experiments I think I am pretty well justified in inferring that we are in a position to *render bromide of silver sensitive for any colour we choose*—that is to say, to heighten for particular colours the sensibility it was originally endowed with. To effect this, all that is required is to add thereto some substance which promotes the chemical decomposition of the bromide of silver and absorbs the particular colour in question but not the others. Perhaps we may even arrive at this, namely photographing the ultra-red as we have already photographed the ultra-violet. The photographic inefficiency of certain colours, which has hitherto proved so great an obstacle, would in that case be surmounted. The following experiment shows to what extent this is practically substantiated. A photograph was taken of a band of blue upon a yellow ground. Employing an ordinary collodion plate with iodide of silver, I obtained thereupon a white band upon a black ground. On a bromide-of-silver and collodion plate, where the action of the blue and the yellow is equally powerful, it was to be anticipated that no effect was to be obtained. I therefore placed in front of the object-glass a disk of yellow glass, which absorbed the blue light and transmitted the yellow unimpaired; and then, after an exposition of suitable duration, I obtained in point of fact a dark band on a light ground.

The matter is not alone of practical but also of scientific in-

terest. Hitherto it has been held that the haloid salts of silver were modified chemically only by the rays which they absorb to a marked extent (Schultz-Sellack, 'Reports of the German Chemical Society,' 1871, p. 211); and, moreover, the influence of "sensitizers" has been partially called in question (Schultz-Sellack, 'Photographic Communications,' seventh annual publication, p. 301).

My investigations show that, with respect to the sensibility of photographic plates to the action of light, not only does the optical aptitude for absorption on the part of the sensitive silver salts themselves, but also the optical aptitude for absorption on the part of the substances mixed therewith play a prominent part.

Further experiments on this point are in progress.

Berlin, October 1873.

XXXIII. *A Theory of the Effects produced by Fog and Vapour in the Atmosphere on the Intensity of Sound.* By Professor CHALLIS, M.A., F.R.S., F.R.A.S.*

THE experimental results obtained by Dr. Tyndall relative to the intensity of sound under various conditions of the atmosphere (as published in the Proceedings of the Royal Society, No. 149, pp. 58-68) are of so much scientific importance that it seemed to me desirable to endeavour to account for them theoretically. The questions they raise are clearly *hydrodynamical*; and I think I shall be able to show that the appropriate answers may substantially be given by reference to propositions in hydrodynamics which I have previously discussed in the pages of this Journal.

It is evident from the experiments that the phenomena to be explained are of two kinds—those resulting from *Fog*, or the presence in the air of vapour in a visible form, and those due to the admixture with the air of *invisible vapour*. As to *Fog*, or *Haze*, I shall at once assume that it is to be attributed to the suspension in the air of extremely minute globules of water. This supposition is justified by phenomena of the *Fog-bow*, which indicates by its form an action of spherical drops of water on light the same as that which takes place in the case of the primary rainbow; and its freedom from colour gives evidence of the extremely small size, as compared with that of rain-drops, of the suspended globules.

Let us conceive aërial vibrations capable of producing sound to be generated and transmitted in air loaded with an immense

* Communicated by the Author.

number of these small globules. The problem of determining the velocity and condensation of the air due to the reaction of a single small sphere on which such vibrations are incident, I have already discussed and applied on several occasions. (See the Number of the Philosophical Magazine for June 1864, pp. 457 & 458, that for October 1865, pp. 262 & 263, and the solution of Example VI. in pp. 279–287 of ‘The Principles of Mathematics and Physics.’) Suppose the velocity at any point due to the reaction of the sphere to be resolved along, and perpendicularly to, the radius vector drawn to the point from the sphere’s centre, and let the former resolved part be U , and be reckoned positive in the direction from the centre, and the other be W , and be reckoned positive when it is in the same direction as that of the incidence; also let σ be the condensation at the same time at the same point. Then if r and θ be polar coordinates of the given point, the origin being at the centre of the sphere and θ being the angle which the radius vector makes with an axis drawn in the direction of incidence from the origin, and if b be the radius of the sphere, and T the velocity of the incident vibration at the time t in a plane through the origin transverse to the axis, the solution of the above-mentioned problem gives

$$U = \frac{Tb^3}{r^3} \cos \theta, \quad W = \frac{Tb^3}{2r^3} \sin \theta, \quad a^2\sigma = \frac{dT}{dt} \frac{b^3}{2r^2} \cos \theta.$$

Now any *reflection* of the incident waves, caused by the reaction of the sphere, will be proportional to $a\sigma$, and must be extremely small on account of the smallness of the factor $\frac{b}{a} \cdot \frac{dT}{dt}$. If, for instance, we suppose that $T = m \sin \frac{2\pi at}{\lambda}$, λ being the breadth of the waves, the maximum value of that factor becomes $\frac{2\pi mb}{\lambda}$, and therefore, by reason of the ratio $\frac{b}{\lambda}$, is excessively small for all such values of m and λ as those which are applicable to the experiments.

Again, on account of the small size of the globule, the factor $\frac{b^2}{2r^2}$ will become excessively small at small distances from its centre, because such distances may still be many multiples of b . Thus, by reason of the two factors combined, the reflection from a single globule may be so extremely minute at all sensible distances from its centre, that the sum of the reflections from a very large number of globules contained within a given finite space might only generate reflex waves of moderate magnitude and coming

from a great *depth*. The experiments of Dr. Tyndall showed that this is actually the case in a foggy or hazy atmosphere; and accordingly this fact, so contrary to previous expectation, may be considered to be accounted for by the present theory.

Taking now into consideration the velocities U and W , it may be proved (as is shown in the discussions above cited) that these velocities are such that just as much fluid crosses any plane perpendicular to the direction of the propagation of the incident waves as would have crossed if the globule had been absent. As this is true with respect to each globule, it follows from the principle of the coexistence of steady motions, that the mean effect of the reaction of all the globules is simply to augment the velocity in the incident waves by contracting the space in which the fluid moves, just as the rate of the flowing of a river is increased by contraction of its channel. By this means an impetus would be given to the sound-waves; and it might thus happen that, in the absence of countervailing causes, sound would be audible in a hazy atmosphere at unusually great distances. This in fact appears to have occurred in the experiments of July 1 and October 30.

During a fall of rain or snow, the drops or the flakes might by their size produce a sensible amount of reflection, and so diminish the intensity of the sound; but, on the other hand, by their occupation of space they would, in the manner explained above in relation to the globules of vapour, give impulses to the vibrations; so that upon the whole (as appears from experiment to be the case) rain or snow falling might have little effect on the intensity of sounds.

It is to be observed that the theory points to no change of the *rate of propagation* of sounds by the action of visible vapour, but exclusively to an alteration of their *intensity*.

Let us now suppose vapour in an *invisible* state to be mixed with the air. The vapour is connected with the air only by a kind of mechanical suspension; yet there is reason from experiment to conclude that the elasticity of the compound differs very little from that of the air itself. So long, therefore, as the mixture is not irregular, there would appear to be no reason for the production of reflected sound. But, to use Dr. Tyndall's terms, an irregular or "flocculent" admixture would render the air "acoustically opaque." This effect may be conceived to take place as follows.

If it were possible that two portions of an elastic fluid of different densities and the same elasticity could be in juxtaposition on opposite sides of the same plane, in any case of the propagation of waves from one portion to the other reflection would take place at the plane of separation, whether

the waves passed from the rarer to the denser portion, or from the denser to the rarer. In an irregular, or flocculent, admixture of air and vapour of water such an abrupt change of density would not be possible; but there might be changes of density more or less rapid from point to point without change of elasticity; and, so far as these changes tend to be abrupt, partial reflection of the waves might ensue. It is also to be considered that the reflections would always take place in the directions of most rapid change of density, whether the density be increasing or decreasing; so that in a flocculent admixture there might be reflections in various directions, and the sound might be dispersed or absorbed in a manner analogous to the absorption of light on its entrance into an opaque or partially transparent medium. Thus air charged with vapour in an invisible state, although optically transparent, might be in various degrees impervious to sound. That this is actually the case was clearly demonstrated by the experiments. This view is confirmed by the fact, observed on October 8, that the intensity of the sound was increased by a heavy fall of rain, the effect of which would be, as is remarked by Dr. Tyndall, to remove to a certain amount by condensation and precipitation the invisible vapour to which the diminution of the intensity of sound is mainly attributable.

Thus theoretical reasons can be given for the experimental results that fog and haze do not diminish, and may even increase, the audibility of sounds, and that loss of sound is chiefly due to certain conditions of the invisible vapour mixed with the air. Theory also indicates that neither by fog and haze, nor by that condition of invisible vapour which diminishes or absorbs sound, is *the rate of propagation* sensibly altered. But I know of no *à priori* means of determining whether the amount of vapour in *regular* admixture with the air may not have some influence on the velocity of sound; and this, I think, is a point which is open to further inquiry by experimental means.

The greater distance to which sounds are audible to leeward than to windward of the place of excitation, is very little owing to the transmission being accelerated by the wind in the former case and retarded by it in the other. It is chiefly due to the circumstance that in those vibratory movements of the air to which the loudness of the sound is principally due there is an excess of condensation above rarefaction, and the consequent excess of the velocity corresponding to the condensation is in the same direction as the current of the wind on the leeward side and opposed to it on the windward side. As the action on the ear is in each case proportional to the *square* of the compound velocity, the result will be an excess of intensity on the leeward side.

In concluding these remarks, I take occasion to advert to the solution I have given, on hydrodynamical principles, of the problem of lines of magnetic force, this problem being in its conditions analogous to that of determining the effect of the small globules of water in a foggy atmosphere on the intensity of sound. (See the "Note on the Hydrodynamical Theory of Magnetism" in the *Philosophical Magazine* for July 1869, pp. 42-45, and arts. 4-8 of a new discussion of the same theory in the Number for June 1872). In that solution I have argued that a stream, or a vibration, of the æther receives an impulse on entering into a magnet by reason of the contraction of channel resulting from the occupation of space by the atoms of the magnet, and that this impulse is proportional to the number of atoms in a given space. On the hypothesis, therefore, that the density of the magnet increases by slow degrees from one end to the other, a given element of the æther will receive successive impulses as it moves towards the denser end, and in this way a permanent stream will be maintained in opposition to the inertia of the fluid. In the case of the transmission of sound in a foggy atmosphere, there is not an analogous increment of the globule-density in the horizontal direction of the propagation; but so far as experiment indicates that an impulse is given to the sound-vibrations by the presence of the globules and the intensity of the sound is thereby increased, the two kinds of phenomena are alike, and the proposed theoretical explanations of them are mutually confirmatory.

Cambridge, March 18, 1874.

XXXIV. *Connexion between Capillary and Electrical Phenomena.*
By GABRIEL LIPPMANN*.

[With a Plate.]

THE present investigation was made in the laboratory of Professor Kirchhoff, to whom I am greatly indebted for his counsel and for his kind assistance. It might well have been difficult to seek *à priori* for relations between electrical variables and the so-called constants of capillarity; and in fact I only gradually arrived at such relations, starting from an experiment for which I am indebted to Professor Kühne of Heidelberg, and which is essentially as follows.

A drop of mercury is introduced into dilute sulphuric acid in which is dissolved a trace of bichromate of potash; a polished

* Translated from Poggendorff's *Annalen*, vol. cxlix. p. 546.

[Note.—In a paper by Mr. C. F. Varley, read before the Royal Society, January 12, 1871, and published in this Journal, vol. xli. p. 310, some of the results contained in this paper have been anticipated.—ED. PHIL. MAG.]

iron wire is fixed near it so as to dip in the acid and just touch the edge of the drop of mercury. As soon as contact has taken place, the drop performs regular vibrations which may last for hours. The resemblance between this phenomenon and the movements of mercury electrodes (see Wiedemann, *Galvanismus*, 1872, § 368) is striking, and the explanation is manifestly the same. From the view hitherto received it would be as follows. The solution containing chromic acid would oxidize the surface of the drop and thus flatten it. In contact with iron an iron-mercury couple is formed. The current would reduce the surface by electrolysis; the drop would contract, and the contact with the iron would be broken; the same succession of phenomena would ensue as before, and so forth. By using a sufficiently concentrated chromic acid solution these processes are actually seen to take place; but with a dilute solution the surface always remains bright. Measurements have shown in fact that the polarization of mercury by hydrogen effects a contraction, and that we need only remember the depolarizing action of chromic acid to explain the above phenomenon.

Experiments which I will now detail more completely have shown that the capillary constant (*superficial tension, coefficient of Laplace's formula*) at the contact-surface of mercury and dilute sulphuric acid is an invariable function of the electromotive force of the polarization at the same surface.

I. Variation of the Capillary Constant with the Electromotive Force of the Polarization.

Measurements.—The apparatus consisted of a vertical calibrated glass tube, G G', which was connected at the bottom by means of an india-rubber tube with a reservoir of mercury, A (Plate II. fig. 1). The mercury rose thus in the tube G G', but underwent there a capillary depression which was measured with the cathetometer, and from which the capillary constant could be determined in the ordinary manner. The upper part of the glass tube was filled with dilute sulphuric acid (one eleventh of the volume of acid), which wetted the meniscus of the mercury M, and was continued by the glass siphon, H, into the glass vessel, B, which also contained dilute sulphuric acid. At the bottom of the vessel was a layer of mercury, B, to serve as a second electrode. The capillary depression of the mercury in the tube G G' was, of course, corrected for the pressure of the dilute acid. In order to evoke in M a known E. F. P. (Electromotive Force of Polarization), the two masses of mercury (namely, that in B and the mass A M) were respectively connected with two points, P and Q, of the circuit of a Daniell's element by means of the platinum wires α, β , which may be called the poles of the appa-

ratus. A branch current then traversed the apparatus, which now acted as a decomposing cell, until the E. F. P. produced was equal to the difference of potentials between P and Q. The E. F. P. bears the same relation to the electromotive force of a Daniell as the resistance P Q bears to the resistance of the entire circuit. This relation may be deduced from the deflection of a tangent-compass contained in this circuit. The ratio of the surfaces of mercury in M and in B was intentionally taken very small, in order that the electromotive force of polarization in M need alone be taken into account; for it is clear that a quantity of electricity which is sufficient to produce in M any given hydrogen polarization would give on the surface B, which is many thousand times as large, no appreciable oxygen polarization. Thus was obtained from the indications of the compass the E. F. P. in M, and from the indications of the cathetometer the simultaneous capillary constant. In order to reduce the E. F. P. in M to zero, all that was necessary was to interpose a simple metallic circuit between α and β . The magnitudes to be measured are not small. Thus the depression in a tube having a radius = 0.32 millim. is 14 millims. for an electromotive force of polarization = 0; for an electromotive force equal to one Daniell it is 18.90 millims.; the alteration in level is therefore equal to 4.90 millims. = 0.35 of the original depression. The constant of capillarity, therefore, is equal to 30.4 for an electromotive force equal to 0; and is equal to 40.6 for an electromotive force of polarization equal to one Daniell.

In order to measure the alterations in the constant of capillarity with still greater accuracy, instead of the tube G G' an extremely hollow open point $\gamma\gamma$ (fig. 2) was used, which was prepared by drawing out one end of a glass tube. Mercury was poured into this tube to such a height that it penetrated into the fine point and partially filled it (fig. 2). The point dipped in dilute sulphuric acid; the bubble of air which at first occupied the end was removed by pressing out some mercury. There was thus in the point a wetted hemispherical* meniscus M of about $\frac{1}{100}$ millim. radius, whose capillary pressure counterbalanced the pressure of the mercury contained in the tube (750 millims. in height). The dilute acid was also in contact with a second mass of mercury, B, which, as before, was to serve as positive electrode; by means of platinum wires both masses could be connected with the external poles α, β . These poles were first placed in metallic connexion with each other, whereby the E. F. P. in M was equal to zero, and a microscope so placed in front of the point that with a 220-fold magnification one of the cross-threads was in

* The angle which mercury forms with glass under dilute sulphuric acid is always zero.

the tangent plane of the meniscus (fig. 3). To make a measurement, a Daniell's element, for instance, was interposed between α and β : the column of mercury disappeared from the field of view; and in order to adjust the mercury to the thread a new pressure, which might be called the *compensating pressure*, had to be exerted upon the column of mercury which supported the meniscus. This compensating pressure bears to the former pressure the same ratio that the increase of the constant of capillarity at the meniscus does to the former value of this magnitude. This is clear from Laplace's formula, in which the curvature must be assumed to be constant when the adjustment of the meniscus is constant, and thus the pressure is proportional to the constant of capillarity. In the example given the compensating pressure amounts to 260 millims. of mercury (more than one third of an atmosphere) when a Daniell's element is interposed—that is, 0.35 of the former pressure (750). The constant of capillarity has increased by 0.35 of its former value.

The compensating pressure was exerted by means of compressed air, produced and measured by means of an air-pump and of a mercury manometer. Electromotive forces of polarization which were equal to known fractions of a Daniell were produced by the above-described method of branch currents. It was thus found that to every value of the E. F. P. a perfectly definite value of the constant of capillarity corresponds; so that from the one magnitude may be inferred the value of the other.

In *all* of these experiments in which the circuit was closed (that is, where between α and β either a mere wire or a constant electromotive force was interposed), it was surprising to observe how constant were the results—that is, the constant of capillarity and the invariability of the position of equilibrium of the meniscus. We have been accustomed to certain “disturbances” which occur in capillary experiments arranged in the ordinary manner (that is, without completion of the electrical circuit), and which, of course, occurred here also when α and β were insulated from each other. These disturbances are as follows: (1) the position of equilibrium is different according to the direction of the previous motion of the mercury; (2) it may suddenly change on agitation, as, for instance, by tapping; (3) the position of equilibrium may slowly change with time and only cease to become displaced hours after. But when the circuit was closed by interposing, for instance, a wire between α and β , all irregularities at once disappeared and they could not be reproduced; that is, the position of equilibrium became so constant that the meniscus was at once adjusted to the cross-wire with a precision which, in spite of the 220-fold magnification, left nothing to be desired. Moreover the cause of these disturbances was discovered (p. 290).

The Capillary Electrometer.

Since, in the apparatus just described, every compensating pressure is represented by a definite value of the electromotive force interposed between α and β , the latter can be inferred from the former when once a table is constructed for this purpose. Thus the free electrical tension at the poles of a Daniell was measured by the compensating pressure of 260 millims. mercury, and smaller electromotive forces by smaller pressures. The apparatus in this case is not traversed by a current; for the electromotive force of polarization holds in equilibrium the electromotive force between α and β . It is only necessary to have this electromotive force so small that it produces no appreciable decomposition of water; it must not much exceed one Daniell. This can always be attained if a known number of Daniell's elements be allowed to act against the electromotive force to be tested. The capillary electrometer is also very sensitive in regard to quantities of electricity; to charge it is to produce an appreciable polarization upon a meniscus the surface of which is equal to $(2\pi \times \frac{1}{100})^2 = 0.0006$ sq. millim. And in fact a deflection is obtained when a feebly charged proof plate from a Coulomb's balance is approached to the pole α —or if the current of a Daniell be momentarily closed by the apparatus, the current being enfeebled by the interposition of a piece of air-dried glass. The sensitiveness of such an electrometer is therefore so great that that of no other electrometer, excepting that of Thomson, can be compared with it. This sensitiveness is moreover constant at all times; the deflections are apparently momentary, in most cases at least; and the deadening is very rapid. The pole β must be in metallic connexion with the gas- or water-pipes, while α is carefully insulated from the table by means of shellac supports. If also α remains insulated from β for some time, the apparatus charges itself, as is also observed in Thomson's electrometer. Accidental contacts, atmospheric electricity, &c. produce these charges.

When the indications of an atmospheric-pressure manometer are sufficient for measuring the pressure, the electrometer may be arranged in a form which is easily transportable. The drawn-out glass tube is only a few centimetres long, and is connected with a second tube sealed at one end, which contains mercury and compressed air, and acts as a manometer for atmospheric pressure. This connexion is established by means of a short thick caoutchouc tube, which is under a small screw-press to produce the necessary pressure. The whole is placed upon a small wooden bracket, which may be clamped to the stage of any microscope. The indications of the instrument are quite constant.

Also, instead of applying compensating pressure, successful attempts were made to measure micrometrically the displacement of the meniscus which took place each time. The drawn-out tube is then fused-to at the outer end, and contains a quantity of compressed air, which produces the necessary constant pressure. The electrometer is then a few centimetres in length; it must, however, be empirically graduated. It is best when the electrometric glass point presses like a spring against the side of the tube which contains the dilute acid, so that under all magnifying-powers it may be conveniently observed.

Electrocapillary Engine, fig. 5.

Just as electromagnetic motors have been constructed which, depending on the principles of electromagnetism, derive their work from the battery and are driven by magnetic forces, so also an electrocapillary motor has been constructed which is driven by capillary forces, and transforms electrical into mechanical work. Such a motor, which is now in the Physical Institute at Heidelberg, consists of a glass trough, K K, filled with dilute sulphuric acid (15 per cent. by volume of acid), and containing two glass vessels, G, G₁, which are partly filled with mercury. These masses of mercury, G, G₁, can severally be connected with the poles of a Daniell's battery, D, by platinum wires, the upper parts of which are kept from contact with the mercury by means of fine glass tubes. In the circuit there is a commutator, W, by which these connexions may be reversed, so that each mass of mercury may be successively polarized with hydrogen. On each mass of mercury floats a bundle of glass tubes, B, B₁, which are about 2 millims. in diameter and are open both at the top and at the bottom. About 300 of these tubes form a vertical bundle 60 millims. in height and of the same diameter, and are held together by platinum wire. In the axis of the bundle a glass rod is fastened, which plays the part of the stem of a piston. The lower half of the bundle floats in the mercury, while the upper is immersed in the acid and is quite filled with it. The bundle floats freely upon the mercury and remains vertical because the glass rod is clamped in a metal U-shaped rod, the lower ends of which are not free but fixed to the lower surface of a double horizontal lever, which is movable about a fixed horizontal axis, and which can oscillate like a scale-beam. Thus the bundles may be said to hang to it upwards as the scale pans downwards, and keep each other in equilibrium. By means of the vertical stirrup V, the rod s, and the bent lever z, the oscillating motion of the lever may be converted into a rotating one of the flywheel R; on the axle of the flywheel is another bent lever z, which moves the commutator. The whole has a cer-

tain resemblance to some marine steam-engines. If, now, by means of the binding-screws σ, σ , the whole be connected with a Daniell's element, one mass of mercury is polarized with hydrogen and the other with oxygen. On the first side the capillary constant, the capillary depression in and between the tubes, and thus also the force by which the bundle is driven upwards is increased by 0.35 of its value, and this bundle rises; exactly the reverse is the case on the other side. The wheel begins to turn; after a complete excursion, the current is reversed by the commutator and the action begins again in the opposite direction.

The machine works silently, without sparks, and with very feeble currents, therefore very economically. It once worked five days and nights with a single Daniell's cell. The number of turns, which was 108 in a minute, of course decreases when the sulphate of copper is exhausted*.

The working-power of such a machine, which is driven by capillary forces, is entirely independent of the volume which it occupies, and depends alone on the variation of the surfaces of contact of the two liquids. This may be readily calculated from the well-known law, that the work of capillary forces is proportional to the variation of the surface and is independent of its shape. If, for instance, the E. F. P. of the surface of mercury has successively the values 0 and one Daniell, it may be easily calculated from the numbers above given that the work is equal to

$$0.01 \times S \text{ kilogrammetres,}$$

if S is the variation of the surface in square metres.

II. *Disengagement of Electricity and Polarization with Capillary Phenomena.*

Experiment has shown that capillary phenomena allow us to transform mechanical work into electrical. Any arrangement which, when a current passes, produces a motion can conversely serve as electromotor. Helmholtz has deduced this reciprocity for electromagnetism and induction from the principle of the conservation of force; his demonstration may without much trouble be put into a somewhat more general form. We have here apparently something similar to induction.

If, for instance, instead of the battery a galvanometer be connected with the electrocapillary engine which has just been described, and the wheel be turned by the hand, it will be seen that the needle deflects; the deflection continues as long as the turning of the wheel; the direction of the deflection changes with the direction of the rotation.

* This machine, as well as the above-named electrometer, is made by M. Jung, mechanic, of Heidelberg.

These currents may be produced and measured in a simple manner.

The apparatus described above (page 285) was used for the measurement. The poles α and β were respectively connected with a reflecting galvanometer of 2000 turns. To make an experiment, the reservoir of mercury A was either raised or lowered; the level of the mercury G G' was altered thereby, and a throw of the needle produced. Alterations in level of a few millimetres were sufficient to make measurements; otherwise currents are obtained so strong that a needle which is not astatic strikes against the stops. When the level is raised, the surface of contact of the mercury and the acid in M increases. The galvanometer then indicates a current which proceeds through the dilute sulphuric acid from the increasing electrode to the other. On sinking, the direction of the current is reversed.

Experiments were made with various heights of lift and with different glass tubes. Let α be the observed deflection of the galvanometer, l the height of the lift, r the radius of the tube; all the numbers found satisfied the condition

$$\frac{\alpha}{2\pi r l} = \text{const.} = \alpha_0;$$

that is, *the quantity of electricity disengaged is proportional to the increase of surface, and independent of the form.*

This quantity of electricity was now determined in absolute electromagnetic measure. The above constant α_0 denotes the deflection in scale-divisions for an increase of surface of 1 square millim. It was found that $\alpha_0 = 0.55$ on the scale. To express this number in electromagnetic measure, the ends of the galvanometer wire were connected with the ends of a coil above which a second coil was placed; this latter was traversed by a current whose intensity amounted to 4.5 in absolute electromagnetic measure. The potential of the two coils on each other (as given by Professor Kirchhoff) amounted to 23,223,000. When the current in the second coil was opened, an induced current was produced in the first which deflected the mirror through 57 divisions. Moreover the resistance of the galvanometer together with the induced coil was compared with a known resistance by means of Wheatstone's bridge, and was found to be equal to 78×10^{10} divisions. Hence the quantity of electricity for a one-division deflection amounted to

$$\frac{23223000 \times 4.5}{78 \times 10^{10} \times 57};$$

and thus for a deflection of 0.55, or an increase in surface of 1 square millim., the quantity of electricity disengaged is in abso-

lute electromagnetic measure

$$q = \frac{23223000 \times 4.5 \times 0.55}{78 \times 10^{10} \times 57} = \frac{12927}{10^{10}}.$$

To make this clearer we may call to mind that, according to Weber, the electrical equivalent of water is 0.0098 milligr. Hence an increase of surface of 1 square millim. would disengage a quantity of electricity which would decompose almost 130 milligrammes of water.

In order to have the deflections proportional to the quantities of electricity, the currents must, of course, have an infinitely short duration compared with that of the swing of the needle. With this view the reservoir of mercury was provided with a cock, which was only opened for a short time after a complete raising or lowering. Moreover small displacements were employed; for the current requires an appreciable time if, with a given glass tube, the displacement of the mercury exceeds a certain amount, because with this length the resistance of the thin layer of acid between the glass and the mercury, and which forms a part of the circuit, becomes larger, and the discharge slower.

The same electrical currents may also be very simply produced. A glass vessel contains mercury and dilute sulphuric acid. A glass funnel filled with mercury is so adjusted that its lower very fine aperture dips in the acid. If the two masses of mercury (that in the glass vessel and that in the funnel) are respectively connected with the terminals of a galvanometer, the needle is deflected as long as the flow of mercury continues. The extension of surface of each drop as it forms causes the production of the current.

The funnel may be replaced by a drawn-out tube, and the vessel by a second similar tube, and thus the simple electromotor of fig. 4 may be constructed. The mercury then flows through both tubes; when the stationary condition has been attained, the deflection of the needle remains constant.

Polarization by Capillary Forces.

If, after the metallic contact between the poles α and β has been broken, the mercury be displaced, we have the ordinary condition of things; for hitherto no attempt has been made to complete an electrical circuit in capillary experiments. Phenomena are then observed of which a portion are known as "unexplained disturbances." If the reservoir A is raised (that is, the surface in M increased), and at the same time the pole β

put in communication with the earth, the wire α becomes charged with free negative electricity, as was established by means of Thomson's electrometer; the deflection of the electrometer may become as great as if its poles had been connected with a Daniell's element. It is at the same time observed that the depression of the mercury in the glass tube is greater than when the circuit is closed; in other words, the constant of capillarity is greater than before. Now the increase of the electromotive force between mercury and dilute acid, with a simultaneous increase of the constant of capillarity at the common surface, is just what is called polarization by hydrogen: the phenomena can then be thus represented:—

If by mechanical means the surface of contact between mercury and acid water be increased, the mercury thereby becomes polarized with hydrogen.

On this may be based a very striking experiment which may be made with the capillary electrometer. If by blowing or by suction with the mouth the atmospheric pressure over the mercurial column be varied, the column of mercury in the fine point can be moved without exertion—this, however, only so long as there is metallic contact between α and β . If this contact be suddenly broken, the column of mercury at once becomes immovable, as if it were frozen. This may be thus explained. On blowing, for instance, the surface of mercury begins to increase; it thereby becomes polarized, and the increase of the capillary constant produces an increase of the capillary pressure which cannot be overcome by the lungs. The exact opposite is the case with suction.

On the same phenomena is based an apparent disturbance observed in capillary experiments, namely a slow decrease of the constant of capillarity, as has been shown by Quincke for mercury in water. If, for instance, mercury be allowed to ascend in a moistened capillary tube, the surface of the mercury in the tube is increased and is thereby polarized. Now polarization, as is well known, diminishes with time, first rapidly, afterwards continually more slowly. In accordance with this a decrease of the constant of capillarity is observed. A similar thing happens when a drop of mercury is brought into water: in flattening on the bottom it increases its surface and assumes a slowly decreasing polarization.

With the electrometer or galvanometer it can be directly shown that when mercury is contained in a glass vessel under dilute sulphuric acid, dipping a glass rod into it or merely inclining the vessel is sufficient to produce a change of the capillary and electrical conditions. Similarly any agitation produces an alteration of the capillary constant. But when a

circuit is closed, the polarization, capillary constant, and depression are constant.

By "increase of the surface of contact" of mercury and dilute acid two things might be understood—(1) moistening fresh parts of the mercury which had hitherto been dry, or (2) further recession from each other of parts already wetted. In all the above phenomena the *second* is to be understood. This can be shown by the following simple experiment.

On a dry surface of mercury a broad drop of dilute sulphuric acid is placed, which is then removed by a pipette, so that only a wet spot remains. If this spot be perforated by an iron point it polarizes and contracts immediately. But we only see a distortion of the entire surface, such as would take place upon the surface of a stretched caoutchouc balloon upon which was a damp spot and from which air emerged. The individual details at what is ordinarily the jagged edge of the spot, as well as the outlined part of the dry surface, remain separately recognizable during the distortion, as if they had been drawn upon a caoutchouc membrane; and after the polarization has ceased they return to their old position.

On Young's view, according to which the constant of capillarity is regarded as a superficial tension, the result last adduced (namely, that when the circuit is open the constant increases on expansion) would be simply expressed thus—that the surface of mercury behaves like an ordinary elastic membrane, the tension of which increases when the membrane is stretched.

XXXV. *On the Measure of Work in the Theory of Energy.* By ROBERT MOON, M.A., *Honorary Fellow of Queen's College, Cambridge**.

TO my former remarks on this subject† I desire to add the following.

1. I cite the following sentences of Professor Maxwell as expressing generally accepted views:—

"Work is done when resistance is overcome; and the quantity of work done is measured by the product of the resisting force

* Communicated by the Author.

† See 'Philosophical Magazine' for September 1873.

and the distance throughout which that force is overcome" (Theory of Heat, 1871, p. 87).

" Fs is the work done by the force F acting on the body $[M]$ while it moves in the direction of F through a space s " (ibid. p. 89).

Interpreting the latter of these sentences by the former, it is clear that in the case considered the resistance offered to the action of the force F is treated as exactly equivalent to, and as capable of being represented by, F . The question arises, therefore, What is F ?

The phrases "the absolute force," "the force acting at a point," are so constantly in use among us that we are apt to lose sight of their purely conventional character.

Force can only be measured by its effects; and in order that force may produce any effect, it must operate throughout an interval of time. As a matter of fact F is the amount of momentum generated by a force acting uniformly during a unit of time; and what F truly *represents* is the force acting on a body *during a unit of time**. If, therefore, F can be accepted in any degree as the representative of the resistance offered in the above case, it must represent *the resistance offered in a unit of time*. Thus, if we adopt the above definition of work, it follows from the simple rules of arithmetic, not less than I trust that I have already shown it to result from sound mechanical principles, that the work done by the force F acting during the time T is FT , and not $\frac{FT^2}{2}$ as, if the measure of work above proposed were admissible, it would be, supposing the body to move from rest under the influence of the uniform force F throughout the time T .

2. Suppose a body whose mass is M to be moving in a certain direction with a velocity V_1 , and that the force F is applied to the body in the direction of its motion. Then, as before, if F be uniform we shall have

$$Fs = \frac{1}{2}(MV^2 - MV_1^2),$$

where s represents the space described by the body while the velocity changes from V_1 to V ; and putting $V = V_1 + v$, we shall have

$$\text{work} = Fs = \frac{1}{2}M(v^2 + 2vV_1). \quad \dots \quad (\alpha)$$

We thus arrive at the entirely inadmissible conclusion, that while the body moves through the space s , the work done upon it by the force F , and therefore, according to the above

* The test of uniformity, and necessarily the only test, being that the force generates equal amounts of momentum in equal times.

measure of work, the resistance offered by the body to the action of the force, will be *greater* when the body has an initial velocity V_1 urging it in the direction *in which it is solicited* by the force (in which case V_1 and v have the same sign), than it has when the body moves from rest,—nay, more, that when the space s through which the body moves (and therefore v) is indefinitely small, the resistance offered in the former case (which will be MvV_1) will be *infinitely greater* than in the latter, when it will be $\frac{1}{2}Mv^2$.

3. The vice, or rather one vice of the measure of work above proposed, is manifest from the formula (α) of the preceding article. In the production of what is there offered as the work done by the force, it is clear that the initial velocity (that which the body had before the force began its work) has been an agent.

This view of the subject may be further illustrated as follows. Suppose that V_1 is the velocity of the body at the beginning of the time T , and that throughout T the motion of the body is counteracted by the uniform force F , which is of such amount as to reduce the body to rest at the end of T . Under these circumstances two things will have been done during the time T :—

- (1) Space will have been described.
- (2) Velocity will have been destroyed.

The space manifestly has not been described by the agency of the force, the only product of which is the destruction of velocity; in other words, V_1 (in this case $= FT$) measures the work done by the force.

4. It may be an object of curiosity to some of my readers to know how the above measure of work came to be adopted. On this point I am sorry to be unable to afford them very precise information. If my memory serves me rightly, Helmholtz, in his paper in Taylor's 'Scientific Memoirs' for 1852, treats it as a thing "well known;" and in Thomson and Tait's 'Natural Philosophy' it appears to be regarded in the same light. Professor Maxwell informs us that "If a body whose mass is one pound is lifted one foot high in opposition to the force of gravity, a certain amount of work is done, and this quantity is known among engineers as a foot-pound."

If this passage may be taken to afford the true clue, we can hardly fail to be struck with the originality of the suggestion that a great philosophical principle may be established by "engineering evidence."

5. I have already said that "force can only be measured by its effects," which in popular language is tantamount to the assertion that force is measured by the work it does. The establishment of the foregoing measure of work, therefore, is in

point of fact a rehabilitation of the Leibnitzian measure of force—a rehabilitation, indeed, of a very peculiar character; for the Leibnitzian measure is not supposed to supplant, but to stand *pari gradu* with its Newtonian rival.

If the views which I have endeavoured to unfold are correct, it will follow that the received measure of work, of whatever use it may be in simple cases (say when a body moves from rest) as affording within certain limits some rude indications, must be regarded as utterly inadmissible for every scientific purpose.

6 New Square, Lincoln's Inn,
February 26, 1874.

XXXVI. *Determination of the Absolute Value of the Siemens Mercury Unit of Electrical Resistance.* By F. KOHLRAUSCH*.

THE more firmly the general adoption of the Siemens Mercury Unit is established, the more desirable is it that the ratio which it bears to the scientific standard of resistance, the so-called Absolute or Weber's unit, should be known as accurately as possible. A determination of this ratio forms the subject of the present communication. It was carried out in the year 1869 in the Magnetic Observatory at Göttingen, by means of, on the one hand, the splendid instruments for galvanic and terrestrial-magnetic measurements which the Observatory and Physical Institute of Göttingen have acquired through the labours of W. Weber, and, on the other hand, of mercury units which Mr. Siemens kindly had prepared specially for this purpose.

I may be allowed to premise that I have shunned no labour in order to reach the degree of precision attainable with such means; and in assuming that this degree of precision has really been reached, I believe I am warranted by years of practice in observations of this nature, no less than by the final agreement of the results.

The outcome of the investigation is that the Siemens unit is equal, in absolute measure, to

$$9717000000 \frac{\text{mm.}}{\text{sec.}},$$

or, expressing the same thing by a more convenient number, to

$$0.9717 \frac{\text{earth-quadrant}}{\text{second}}.$$

* Translated from Poggendorff's *Annalen*, Ergänzungsband VI. S. 1, an abstract having been communicated to the Göttingen Academy of Sciences on the 5th November, 1870.

So far as space allows, I will communicate the observations in such a form that the calculation can be checked, and that it may be possible to form an opinion as to the degree of accuracy that has been reached. Independently of the consideration that, in the case of any fundamental and difficult measurement which lays claim to precision, the fullest detail is desirable in the publication of the numbers obtained, there are in the present case two additional reasons for this course. In the first place, the observations relate to a perfectly new and important principle of galvanometry established by Weber, in connexion with which the methods of observation with a swinging needle, likewise introduced by him but not widely adopted as yet, have been employed. In the second place, my results render probable an error of about 2 per cent. in the unit of resistance adopted by the British Association; and, in view of the authority attaching to the Committee on Electrical Standards, which was appointed by so eminent a body and composed of the first scientific names, and which moreover disposed of considerable external means, there seems need for the greatest caution, but also in the end for more distinct emphasis. A series of objections will be found pointed out further on against the determination of the British Association.

I. On the various Methods for the Absolute Measurement of Resistance.

The determination of the absolute resistance of a conductor by magnetic means always leads to the problem of measuring the current which a known electromotive force produces in the conductor, the unit current being taken as that which exerts externally the unit of magnetic action, and the unit of electromotive force being that which is developed when a conductor is moved against magnetic forces under certain normal conditions*. We owe to Weber four methods for making such determinations of resistance, the principles of which may be briefly stated as follows:—

In the first†, the electromotive force induced by the earth's magnetism in a moving conductor (earth-induction apparatus) of known dimensions is made use of; and the strength of the current is found from the deflections of a short magnetic needle surrounded by a multiplier-coil, also of known dimensions. We only require to know in addition the time of vibration of the

* *Abhandl. d. K. Sächsischen Gesellsch. d. Wissenschaften*, 1846, vol. i. p. 219.

† *Tom. cit.* p. 226.

needle; the magnetic intensity of the earth is not required, as its effects compensate each other. It is, however, essential that the needle should be short in comparison with the diameter of the multiplier; and consequently either the *observations must be made with a small needle, or the multiplier must be constructed of very considerable dimensions.*

The second method*, which is the one employed in the following investigation, is a modification of the first, called for by the very difficulties just referred to. A multiplier with narrow coils and astatic needle serves as galvanometer, the dimensions of which may be made to fulfil the conditions of greatest sensitiveness, or those prescribed by any other considerations, in whatever degree we please. This is possible, inasmuch as the action of the unit current in the multiplier on the needle is not calculated from the dimensions, but is ascertained empirically, according to the laws of magneto-induction, by the *damping* effect which the multiplier exerts on the swing of the needle. But in addition it is now needful to know, not only the time of vibration, but also the *moment of inertia of the needle*, as well as the *absolute value of that component of the earth's magnetic force which acts on the inductor.*

The third method† is distinguished by great simplicity in the necessary instrumental appliances, which consist simply of a multiplier with a magnetic needle vibrating within it. If the time of vibration of the needle is known, and if both the ratio of the magnetic moment of the needle to the earth's magnetic force and the *distribution* of magnetism in the needle have been ascertained by observing its power of deflecting a compass, it is possible from these data and the dimensions of the multiplier to calculate the electromotive force developed in the latter by the motion of the needle. The strength of the current thus produced, and consequently the resistance of the multiplier, is found by observations of its *damping* effect.

The last process‡ consists in setting a multiplier of known dimensions in *rapid and uniform rotation*, and observing the deflection of a small magnetic needle suspended within it.

In each of these methods, whenever great accuracy is aimed at, we encounter difficulties of execution, by no means inconsiderable, arising from sources to which attention is called by italics in the foregoing paragraphs. We must next consider somewhat more in detail the last-mentioned process of the rotating multi-

* *Abhandl. d. K. Gesellsch. d. Wissensch. zu Göttingen*, 1862, vol. x. p. 20. Also printed separately under the title "*Zur Galvanometrie.*" Göttingen, 1862.

† *Abh. d. K. Sächs. Ges. d. Wiss.* 1846, vol. i. p. 232.

‡ Weber, *Zur Galvanometrie*, p. 12.

plier, since this was the process employed in the construction of the English unit of resistance.

II. *On the Determination of the Absolute Unit of Resistance carried out at the instance of the British Association*.*

The fourth method was proposed by Weber in two different modifications: in one the axis of rotation of the inductor is horizontal; in the other it is vertical. In the former case it is necessary to know the *ratio of the two components of the earth's magnetic force*, inasmuch as the horizontal component acts on the needle, while the inductive action is due to the vertical component. By the second modification of the method, which was the one adopted by the Electrical Standard Committee of the British Association, this knowledge may be dispensed with, since the horizontal component occurs, in consequence of its inductive action, in the numerator of the expression for the strength of the current, and as acting on the needle it enters into the denominator; so that we have here what is in truth a most elegant, and, so far as the principle is concerned, a very simple process, independent both of the earth's magnetism and of that of the needle, and presenting only the mechanical difficulty of the production of a constant and rapid motion of rotation.

But, on the other hand, another obstacle presents itself in practically carrying out the second modification, the effect of which comes out most clearly in the work of the "Committee," and appears to call for more serious consideration than the necessity of making a determination of dip. *In this arrangement the suspended needle, as well as the earth's magnetism, exerts an inductive action; and consequently it is needful to eliminate the effect of the former.* If a small magnet, such as is commonly used for galvanometric measurements, were to be employed in conjunction with the multiplier of 300 millims. diameter adopted by the Committee, its own inductive action would far exceed that of the earth's magnetism. If the effect of the former was to be treated as a small correction, it was accordingly essential to use an unusually weak magnetic needle.

The Committee went in reality very far in this direction—so far, indeed, that such a weak magnetic needle as they employed has assuredly never been used for any other measurement. Their magnet consisted of a steel sphere (thus a mass of magnetically the most unfavourable shape possible) of 8 millims. diameter and

* Report of the British Association, 1862, p. 125; 1863, p. 111; 1864, p. 345; 1865, p. 308. Where merely the number of the *page* is quoted in what follows, the reference is to the Report for 1863.

weighing about 2 grammes. But, in addition, this little sphere was purposely weakly magnetized and did not possess more magnetism than can be imparted to a sewing-needle of the mass of $\frac{1}{10}$ of a gramme, as I have assured myself by experiment*. It may serve further to illustrate the figures to state that a small elongated iron rod of 10 grammes, held in the direction of dip, would acquire the above degree of magnetization through the inductive action of the earth's magnetism. A single silk fibre, 2 metres long, was required as the suspending-thread for the steel sphere, in order to reduce the torsion to the small amount prescribed by the varying elasticity and elastic reaction of the silk. Now let a magnetized sewing-needle of the very smallest size be conceived of as fastened by a connecting-piece about $\frac{1}{4}$ metre in length to a mirror of 30 millims. diameter, and therefore (according to the drawing) offering a surface of about 14 square centims. for the action of air-currents, which, even in a well-closed case, cannot be entirely avoided; further, let the whole mass have such a moment of inertia that its time of vibration is ten seconds (p. 173), while that of the sewing-needle by itself would have been about two thirds of a second, and we have an idea of the essential part of the magnetometer acted on by the weak currents of the multiplier, an error of two minutes of arc in the determination of whose position would cause an error of 1 per cent. in the result. Moreover the large multiplier was revolving in the immediate neighbourhood of this magnetometer with a velocity sometimes as high as four revolutions in a second.

It appears to be a defect in the otherwise very complete reports, that, so far as I am aware, there has been no publication of the full details of a series of observations such as would afford ground for accepting or rejecting the objection that has been urged above. It is mentioned (page 174) that individual parts of the lengthy series of experiments were rejected before the calculation was made, on account of their want of agreement with others†; whence it appears that considerable unexplained irregu-

* The steel sphere deflected a compass-needle at a distance of 156.6 mm. through an angle $=\tan^{-1} 0.0078=27'$ (Brit. Assoc. Rep. 1863, p. 172). Hence, taking the horizontal intensity $=1.76$, we get for the magnetic moment, $M=\frac{1}{2} \cdot 1.76 \cdot 156.6^3 \cdot 0.0078=26000$. But since 1 milligr. of steel will assume a maximum of permanent magnetism amounting to about 1000 units (comp. also Schneebeli, *Progr. des Zürich. Polyt.* 1871-72, Pogg. *Annal. Ergänzungsbd.* vi. p. 150), the above degree of magnetization might be given to a thin rod of 26 milligr.

† The German words are *wegen Nicht-Uebereinstimmung mit anderen*, which are not equivalent to those used by the Brit. Assoc. Committee at the place referred to, namely, "any portion of an experiment accidentally vitiated could be rejected by itself."—TRANSL.]

larities were met with. *In physical measurements, however, it is always dangerous to assume that large errors of experiment can only be of accidental origin and are eliminated by a sufficiently great number of observations.*

Indeed, if we now look at the final published results *, they seem to justify some slight degree of hesitation in accepting them. These *mean* values still exhibit differences amongst themselves which rise as high as 1·4 per cent. We find further that the slow spins give a mean result which differs by about 0·5 per cent. from that of the rapid spins. In like manner the publication of a few observations belonging to one and the same day (p. 175) makes it possible to form some opinion. Four results are given, which differ amongst themselves by as much as 2·3 per cent., although each of these numbers is obtained by a series of observations extending over about a quarter of an hour and including about one hundred scale-readings, from which, if need were, the least-accordant numbers had already been struck out. An unprejudiced reader must always have his attention arrested by differences as great as these.

But the discrepancies, amounting in some cases to as much as 8·5 per cent., according as the inductor was turned towards the left or towards the right, are to me totally unintelligible. According to a statement by Mr. Jenkin (Poggend. *Annalen*, vol. cxxvi. p. 387), the explanation of this circumstance lies in the fact that "the thread, to which the magnet was hung, exerted a slight influence in one direction." One is inclined to conclude from this that there was a permanent torsion of the thread one way, whereby the deflection to the two sides would of course be made unequal. But in order to explain such differences as occur here, the torsion would have to be great enough to deflect the magnetic axis of the steel sphere many degrees from the magnetic meridian. It is hardly admissible to suppose that such an oversight was made in suspending the magnet. Should it, however, have occurred, it seems to me that the series of observations affected by it ought to be rejected; for such a want of symmetry would not be willingly allowed even in ordinary working, and must be regarded as specially dangerous when the spherical form and feeble magnetization of the magnet are considered. For to say that the magnetic axis of the magnet, on the permanence of which every thing ultimately depends, was really permanent within minutes of arc, when it did not coincide with the direction of the magnetic directive force, would be a hazardous assertion.

* Brit. Assoc. Rep. 1864, p. 350; Poggend. *Annalen*, vol. cxxvi. p. 386.

Less ground for distrust would no doubt be afforded if the expression that has been quoted were interpreted as meaning that a change had taken place, through gradual elastic yielding, in the position of torsional equilibrium of the cocoon-fibre, as might be the case, for instance, if the fibre had not been suspended for a sufficiently long time. But even this I do not like to suppose; for in such a case the observations ought to have been postponed, or at least the yielding should have been eliminated by means of special observations.

In short, we can hardly avoid coming to the conclusion that the feeble magnetism of the needle gave rise to considerable discrepancies; and it does not appear to me allowable to apply the rules for the calculation of probabilities to such observations unless we have explicit proof of the absence of constant sources of error.

In any case, however, the probable error of 0.1 per cent., which is calculated for the final result, can only apply to the scale-readings; to extend it to the entire measurement is presupposing that there were no other sources of error. Even the very noteworthy agreement between the numbers found in the two years 1863 and 1864, which differ by only 0.16 per cent., cannot be regarded as unconditionally decisive. If we now inquire for other sources of error, we encounter first of all the objection raised by Dr. W. Siemens against calculating the mean radius of the coil from the length of the wire and the number of turns. I think I may conclude, from careful experiments of my own, that this process may be employed without hesitation in the case of *thick* wires; but the section of the wire which we have to deal with here (to judge from the weight and total resistance of the wire, as well as from the dimensions of the coil) cannot have been more than about 1 millim.; so that the above objection may perhaps be regarded as not out of place. But, in any case, the error thus arising cannot have been great.

But possibly another circumstance (which is not alluded to in the Reports of the Committee, though in general the corrections are treated with a comprehensiveness and completeness that is worthy of imitation) may have been of greater importance. The support in which the coil revolved consisted of a "strong brass frame," which, as appears from the figure, formed within itself a closed circuit. It is nowhere stated how the observers convinced themselves of the unimportance of the currents which the closed inductor must have caused in these stationary metallic parts when rotating. No doubt the experimental detection of these currents would have been difficult; but

exactly on this account the neighbourhood of masses of metal ought to excite suspicion.

I have been unable to avoid a criticism of the method by which the "British-Association Unit" was obtained, since the question is how to explain a difference the cause of which I cannot, to the best of my knowledge, find in my measurement. For the rest, it is not impossible that many of the above objections might fall away if the results of the observations were published more in detail; and the want of such publication is the more to be regretted, as it causes a deficiency in the otherwise in some respects classical reports.

The question as to which unit of resistance is best fitted for general use hardly belongs to a scientific investigation. For the sake of Physics itself we can but rejoice in the competition between the Siemens unit and that of the British Association; for thereby the best means is afforded of testing the unalterableness of both, and this is all that comes into account for scientific application.

With regard to practice, the position occupied by Mr. Siemens in relation to telegraphy must have secured a considerable start for his unit; while a no less important circumstance is that the judiciously arranged and, so far as I know, well-adjusted Siemens's Resistance-scales have become widely distributed. Moreover it can scarcely be denied that the definition of the unit by means of mercury is intelligible to practical men, whereas the definition of the other unit would be intelligible to but few in the first instance. But further, a considerable additional weight must be laid in the scale on the side of the Siemens unit, if the other unit, as appears to be the case, does not approach in any important degree more closely to the absolute $\frac{\text{earth-quadrant}}{\text{second}}$

than the mercury unit does by accident. The British-Association unit is in reality (even in the view taken of it by the Committee; comp. Report for 1864, page 346) not an absolute measure but a concrete standard; and for use it is quite immaterial whether its degree of approximation to the absolute unit amounts to 2 per cent. or to 3 per cent. Lastly, if the possibility of reproducing the British-Association unit is to be made to depend, not upon a repetition of the absolute measure (see 'Report' for 1864, page 348), but upon the conductivity of metals, in which case it is self-evident that the preference would be given to mercury, there does not seem to be any reason whatever why a column of mercury of simple and convenient dimensions should not be chosen.

III. *The Absolute Measurement of Resistance by the method of Recoil (Zurückwerfungsmethode).*

The process described at page 296, as the "second" method of determining the absolute resistance of a circuit consisting of an earth-inductor and a galvanometer with narrow coils, depends on the connexion between the coefficient of sensitiveness of the galvanometer and the ratio of damping of the vibrating needle. I intercalate here a demonstration of this principle, as the expression given by Weber* requires a trifling correction.

The coefficient of sensitiveness q is the deflecting moment which a current of unit strength exerts on the galvanometer-needle, when the latter is parallel to the windings of the coil. Then, according to the general laws of induction, the electromotive force which the needle induces in the coil when moving with the angular velocity $\frac{dx}{dt}$ in the neighbourhood of the par-

allel position is $-q \frac{dx}{dt}$. If w is the resistance of the galvanometer, including the path which forms the connexion between the ends of the conducting wire (in the case before us, therefore, w is the resistance of the galvanometer + that of the inductor), the moving needle induces a current of strength $-\frac{q}{w} \frac{dx}{dt}$, and thereby in its turn experiences a deflecting moment $= -\frac{q^2}{w} \frac{dx}{dt}$, which tends to damp its motion.

Besides this galvanic damping, other resistances to motion may exist (such as resistance of the air, &c.) which may be represented by $-c \frac{dx}{dt}$.

Now let the position of equilibrium of the needle be parallel to the windings of the coil; and let the vibrations be so small that, if x denote the angle of deflection at any instant, the deflecting moment due to the earth's magnetism and to the elasticity of the suspending fibre may be represented by $-Dx$. If, lastly, K stands for the moment of inertia of the needle, its motion will be determined by the equation

$$\frac{d^2x}{dt^2} + \frac{1}{K} \left(\frac{q^2}{w} + c \right) \frac{dx}{dt} + \frac{D}{K} x = 0.$$

This is the equation, so often discussed†, of the motion

* *Abhandl. d. Götting. Gesellsch. d. Wissensch.* vol. x. p. 25 (1862).

† Gauss, *Resultate des magn. Vereins*, 1837, p. 74; 1839, p. 55. W. Weber, *Abh. der königl. Sächs. Ges. der Wiss.*, 1846, vol. i. p. 345. Du Bois-Reymond, *Monatsber. der Berl. Akademie*, 1869, p. 807.

of every damped needle, with the difference merely that the coefficients are here given in their physical signification.

Now the coefficient of $\frac{dx}{dt}$ determines, as is well known, the relation between the time of vibration and the amount of damping. Next, let t be the *time of vibration of the damped needle*, and a and b the magnitudes of two successive arcs of vibration, so that $\lambda = \log_0 \frac{a}{b}$ is the *Napierian logarithmic decrement of the needle*. Then we have $\frac{q^2}{wK} + \frac{c}{K} = 2 \frac{\lambda}{t}$.

The second term $\frac{c}{K}$, depending on the resistance of the air, is found exactly in the same way, by observing the *time of vibration* t_0 and the *logarithmic decrement* λ_0 after breaking the *galvanometer circuit*. Then

$$\frac{c}{K} = 2 \frac{\lambda_0}{t_0}.$$

And since t and t_0 are connected by the equation

$$\frac{t^2}{\pi^2 + \lambda^2} = \frac{t_0^2}{\pi^2 + \lambda_0^2},$$

we get finally

$$q^2 = 2 \frac{wK}{t_0} (\lambda \sqrt{\frac{\pi^2 + \lambda_0^2}{\pi^2 + \lambda^2}} - \lambda_0) * \dots \dots \dots (I.)$$

This is the important equation whereby the coefficient of sensitiveness of a galvanometer, with narrow coils and with a needle of any given shape, can be determined from the moment of inertia and time of vibration of the needle taken in connexion with the degree of damping due to the coil, if the absolute resistance of the coil is known. In future this last quantity can be easily ascertained by means of a Siemens's resistance-scale†.

* Weber (*Zur Galvanometrie*, p. 23, 25, where it must be observed that $\frac{q}{K}$ is denoted by f and our λ by λ_1) obtains $q^2 = 2 \frac{wK}{t_0} (\lambda - \lambda_0) \sqrt{\frac{\pi^2 + \lambda_0^2}{\pi^2 + \lambda^2}}$.

† It is hardly needful to say that q is not the coefficient of sensitiveness that comes into account in the case of *permanent currents*. If a permanent current of strength i produces a deflection x , we may put $x = pi$, in which case it is easy to see that $p = q \frac{t_0^2}{\pi^2 K}$. In accordance with the usage frequently adopted in other cases, we may call q the *dynamical* and p the *statical coefficient of sensitiveness*.

In the present case the above equation is to serve to *determine* the resistance w ; consequently a second relation is required in order that q may be eliminated. Such a relation is afforded by the currents produced by an earth-induction apparatus when it is quickly turned, in the ordinary way, from one position at right angles to the magnetic meridian into another at 180° from the former.

Let S denote the *superficial area surrounded by the wire of the inductor*, and T the *earth's horizontal magnetic intensity*, then the quantity of electricity traversing each section of the circuit at each such movement, will be

$$\int idt = 2 \frac{ST}{w}.$$

According to what precedes, this will impart to the needle an angular velocity γ represented by

$$\gamma = \frac{q}{K} \int idt = 2 \frac{qST}{wK},$$

whence we get

$$q^2 = \frac{\gamma^2 w^2 K^2}{4S^2 T^2} \dots \dots \dots (II.)$$

By equating the two values of q^2 given by (I.) and (II.), we get finally for the resistance of the Inductor + Galvanometer, expressed in absolute measure,

$$w = \frac{1}{\gamma^2} \frac{8S^2 T^2}{t_0 K} \left(\lambda \sqrt{\frac{\pi^2 + \lambda_0^2}{\pi^2 + \lambda^2}} - \lambda_0 \right).$$

In order to determine the angular velocity γ , which the separate inductive shocks give to the needle, and the damping λ , Weber employs the *method of Recoil* (*Abhandl. der Königl. Sächsisch. Gesellsch. der Wissensch.* vol. i. p. 349 [1846]), in which inductive shocks in alternately opposite directions are given at every second passage of the needle through its position of equilibrium. If A and B denote the ultimately constant values of the greater and less arcs traversed by the needle swinging under these conditions, we have

$$\lambda = \log_e \frac{A}{B},$$

$$\gamma = \frac{1}{2} \frac{\sqrt{\pi^2 + \lambda_0^2}}{t_0} \frac{A^2 + B^2}{\sqrt{AB}} \left(\frac{B}{A} \right)^{\frac{1}{\pi} \tan^{-1} \frac{\lambda}{\pi}};$$

* Weber, *Zur Galvanometrie*, pp. 16 et seq.

so that the resistance finally becomes

$$w = 32 \frac{S^2 T^2}{K} \frac{t_0}{\sqrt{\pi^2 + \lambda_0^2}} \left(\frac{\lambda}{\sqrt{\pi^2 + \lambda^2}} - \frac{\lambda_0}{\sqrt{\pi^2 + \lambda_0^2}} \right) \frac{AB}{(A^2 + B^2)^2} \left(\frac{A}{B} \right)^{\frac{2}{\pi} \tan^{-1} \frac{\lambda}{\pi}}.$$

IV. Investigation of the possible Errors.

Since λ_0 is always small and can be determined very accurately, and the last factor does not differ greatly from unity, if we bear in mind moreover that λ^2 is always small in comparison with π^2 , in investigating the possible errors we may take the approximate expression

$$w = \frac{32 S^2 T^2 t_0 \lambda}{\pi^2 K} \frac{AB}{(A^2 + B^2)^2}.$$

1. The principal share of the uncertainty must be ascribed to the *horizontal component of the earth's magnetic force* T , the percentage error of which affects the resistance in a twofold degree. The difficulty of accurately determining this element (a difficulty which is no doubt in general underrated) makes it essential that the observations should be carried out in a very completely equipped magnetic observatory. It is evident also that the variations of the earth's magnetism must be taken into account; for the horizontal intensity varies with us to the extent of about $\frac{1}{2}$ per cent.; so that, without observing the variation-apparatus, an error of 1 per cent. *might* arise in the calculation of the resistance.

The observatory of Göttingen without doubt answers to the requirements of the case more completely than any other place, inasmuch as the arrangements that have been carried out there by Weber for determining the horizontal intensity far surpass, in delicacy and convenience, those in use elsewhere. The greatest sources of error in the absolute measurement unquestionably lie in the determination of the moment of inertia and the measurement of distances; in long series of observations of variations the change in the magnetism of the bifilar needle also introduces difficulties that are not inconsiderable. Essential improvements have been effected affecting precisely these points. The distances to be measured are solely those between the suspending fibres of magnets, and therefore can be determined with the utmost accuracy. The weights for determining the moment of inertia are more firmly connected with the magnetometer; they

* In practice, $\sqrt{\pi^2 + \lambda_0^2}$ can always be taken as $=\pi$; and so long as λ is not great, we have as a close approximation (in the present case, for instance, within 0.05 per cent.)

$$w = 32 \frac{S^2 T^2 t_0}{\pi^2 K} (\lambda - \lambda_0) \frac{AB}{(A^2 + B^2)^2} \left(1 + \frac{3}{2} \frac{\lambda^2}{\pi^2} \right).$$

cause the same strain on the suspending-thread in all the observations, and likewise admit of very accurate measurement of the distance between them. Finally, the bifilar magnetometer is supplied with a Weber's auxiliary needle*.

I had myself the gratification of using these arrangements for the first time in their entirety, while determining the terrestrial-magnetic elements at Göttingen in 1867, and of convincing myself of their excellence. During these determinations, and in a subsequent series, I acquired an accurate knowledge of the constants of the instruments and sufficient practice in their use.

While referring to the next section for the observations themselves, I will adduce here two pairs of determinations of intensity, from each of which variations in the terrestrial magnetism are eliminated, and which can therefore be directly compared. The following are these values:—

1867, with an interval of sixteen days, 1·83960 and 1·83849; difference = 0·00011. 1869, with an interval of three days, 1·83860 and 1·83832; difference = 0·00028.

Expressed in terms of the whole, these differences amount to 0·00006 and 0·00015. But the error arising from the moment of inertia and the measurements of distance is not included in these differences; hence for the possible error ΔT of the horizontal intensity we may take about ten times the above amount—that is

$$\frac{\Delta T}{T} = \pm \frac{1}{1000}.$$

2. The *moment of inertia* K of the astatic pair of needles was determined by two different methods. One of them gave $K = 1,135,700,000$; and the other, which was not quite free from objection, 1,132,800,000. We will assign to the former determination the weight two, and assume as the error ΔK the deviation of the two determinations from the mean—that is,

$$\frac{\Delta K}{K} = \pm \frac{1}{750}.$$

3. The *inductor-surface* S is the only magnitude which I have not measured myself. It was determined by Weber when he constructed the inductor; he measured the length of the wire by coiling it upon a large wheel about 3 metres in diameter†. It was moreover controlled by measuring the individual windings. I put the possible error

$$\frac{\Delta S}{S} = \pm \frac{1}{2000}.$$

4. The *time of vibration* t_0 amounted to about 34·4 seconds.

* *Abhandl. d. Götting. Gesellsch. d. Wissensch.* vol. vi. (1855).

† *Abh. der Gött. Ges.* 1853, page 53 of the separate impression.

The greatest difference which occurred between values which ought to agree with each other amounted to 0.018 second. Hence the error assumed is

$$\frac{\Delta t}{t_0} = \pm \frac{1}{2000}.$$

5. Lastly, the arcs of vibration A and B may each include an error in reading of ± 0.2 millim., which observations, to be afterwards communicated, will probably show to be too high. Now, measured in divisions of the scale, $A=370$ millims., $B=225$ millims.; and therefore $\frac{A}{B}=1.64$, $\lambda = \log \text{nat } 1.64 = 0.50$.

If we express the errors produced in w by the separate errors $\Delta T, \Delta S, \dots$ by $\Delta w_T, \Delta w_S, \dots$, we have

$$\frac{\Delta w_T}{w} = 2 \frac{\Delta T}{T} = \pm 0.0020,$$

$$\frac{\Delta w_S}{w} = 2 \frac{\Delta S}{S} = \pm 0.0010,$$

$$\frac{\Delta w_K}{w} = -\frac{\Delta K}{K} = \mp 0.0013,$$

$$\frac{\Delta w_t}{w} = \frac{\Delta t}{t_0} = \pm 0.0005,$$

$$\frac{\Delta w_A}{w} = \frac{\Delta A}{A} \left(1 + \frac{1}{\lambda} - \frac{4}{1 + \frac{B^2}{A^2}} \right) = \pm 0.0000,$$

$$\frac{\Delta w_B}{w} = \frac{\Delta B}{B} \left(1 - \frac{1}{\lambda} - \frac{4}{1 + \frac{B^2}{A^2}} \right) = \mp 0.0019.$$

It will be observed that the ratio of A to B is just of such a kind that a small error in A has no influence.

If we were to add together the whole of the errors in the most unfavourable sense, the total error would be $\frac{\Delta w}{w} = \pm 0.0067$, or $\frac{2}{3}$ per cent. The mean error to be feared in the case of a single determination, or the root of the sum of the squares, amounts to ± 0.0033 , or about $\frac{1}{3}$ per cent.

The error of observation in the *comparison* with Siemens's standard need not be considered; for it does not exceed 0.0001. In order to eliminate the alteration due to change of temperature, the influence of which cannot be directly determined in large masses of wire, one comparison was made at the beginning

of each set of observations, and another at the end. As the temperature of the air changed but little during the time of observation, this method is in any case adequate.

The question arises whether the supposition made in the differential equation (page 302) is correct, namely that the coefficient of sensitiveness q of the galvanometer is constant, or, in other words, that *the damping-ratio is independent of the amplitude of the swing*. In reference to the instruments the question may be thus stated:—Is the multiplier so broad that a lateral displacement of the needles such as occurs in the vibrations does not alter the moment of rotation which a current in the multiplier exerts upon the needles? The question could be easily decided. The horizontal component of the terrestrial magnetism made the largest arc equal to about 3° ; if the damping in these vibrations were already less than in very small ones, its amount must considerably decrease if recoil-observations were made with the vertical component as the inducing force, in which case the arc of vibration amounts to 7° . Observation gave:—

$$\begin{array}{rcl} \text{Large arc} & = 3^\circ, & \text{ratio of damping} = 1.74430 \\ \text{,,} & = 7^\circ, & \text{,,} \text{,,} = 1.74255 \\ \text{Difference} & . & . & . & 0.00175 \end{array}$$

Hence if the induction were executed with the vertical component, a correction would be necessary in our galvanometer which would amount to a quarter of a division in the smaller arc. As the correction must be proportional to the square of the amplitude, it can from this be calculated that for the horizontal component it must amount to about $\frac{1}{4000}$, a magnitude so small that it is not worth while to determine it accurately. In the sequel no regard is paid to it.

As regards the induction on each other of the short currents arising and ceasing in the terrestrial inductor, brief consideration will show that it can exert no appreciable influence on the deflections; for the extra current which accompanies the principal current causes no change in the velocity imparted, but exhibits its influence solely in a sudden displacement of the needle, as I have shown elsewhere*. From the observations there given we cannot estimate the extra current at more than 1.5 division; and that produces an error of at most 0.01 division in our larger arc A , which may well be neglected.

The actual agreement of the three measurements given below quite satisfies expectation. It will be found that they differ respectively from the mean values by 0.14, 0.04, and 0.11 per cent., which would correspond to a "probable error" in the result of 0.05 per cent. In this indeed are not contained the

* Pogg. Ann. vol. cxlii. p. 422.

errors of the moment of inertia and of the inductor surface—nor those of the intensity of the earth's magnetism, except so far as concerns variations in this magnitude. On the other hand, I have assumed sixfold the above amount as the error.

I by no means maintain that with such a limit of error *all* that is desirable is accomplished; but without constructing entirely new instruments, and without erecting special buildings, it would be for the present difficult to exceed this limit.

It must moreover be considered that an alteration of $\frac{1}{2}$ per cent. in the conductivity of the ordinary metals is produced by an alteration of 1° in temperature.

[To be continued.]

XXXVII. *Proceedings of Learned Societies.*

ROYAL SOCIETY.

[Continued from p. 229.]

June 19, 1873.—William Spottiswoode, M.A., Treasurer and Vice-President, in the Chair.

THE following communication was read:—

“On the Action of Electricity on Gases.—No. II. On the Electric Decomposition of Carbonic-Acid Gas.” By Sir B. C. Brodie, Bart., D.C.L., F.R.S.

In my previous experiments the maximum amount of ozone obtained by the action of electricity upon pure oxygen passed through the induction-tube of W. Siemens was about 20 per cent., an amount which, under the conditions of the experiment, could not be exceeded. It occurred to me as possible to replace the 80 per cent. of oxygen unaffected by the action of the electricity by an indifferent gas, and thus to effect the complete conversion of oxygen into ozone. This idea was the starting-point of the following investigation.

When pure and dry carbonic-acid gas is subjected in the induction-tube to the electric action, a certain proportion of the gas is decomposed into carbonic oxide and oxygen, a portion of which appears in the form of ozone. The presence of this ozone may be detected by its odour and the characteristic oxidations produced by it.

It was a point of fundamental importance to determine whether the ozone thus generated was the same in kind as that formed by the action of electricity on pure oxygen, and whether the oxidations referred to were exclusively due to it; for it was quite conceivable that under the peculiar circumstances of the experiment other substances capable of producing similar oxidizing effects might be formed. Now, in the investigation before referred to, I had discovered certain quantitative reactions of ozone by which this substance is discriminated from all other known gases, and by which its presence may be detected and its quantity estimated. The electrized carbonic-acid gas was examined by the aid of these reactions.

When the electrized gas was passed through a solution of hyposulphite of soda, it was found to undergo a diminution in volume equal in amount to twice the "iodine-titre" of the gas, the mean of twelve experiments giving for this value 1.9, the "titre" being 1. The oxidation also effected in the solution of hyposulphite of soda was equal to three times the "titre" of the gas, the mean of a set of five experiments giving for this value, according to one method of estimation, 2.72, and according to a second and more exact method 3.01. Again, the oxidation effected by the electrized gas in a solution of protochloride of tin was equal to three times the "titre" of the gas, the value found as the mean of six experiments being 2.77. In two experiments also, in which both the oxidation and contraction were respectively estimated, 2.75 and 2.69 were the values found for the oxidation, and 1.9 and 1.8 the values found for the contraction, the "titre" of the gas being in all cases assumed as 1. These experiments conclusively demonstrate the identity of the ozone formed by the action of electricity upon carbonic-acid gas with the ozone similarly generated from pure oxygen.

Various experiments were instituted with the view of ascertaining the conditions most favourable to the conversion of a large proportion of this oxygen into ozone. The conclusion at which I arrived was that the greatest proportion of ozone in relation to the total oxygen was produced when a rapid current of dry carbonic-acid gas was acted upon at a low temperature by electricity of feeble tension. When these conditions were realized, it was found that 75 per cent. of the total oxygen eliminated in the induction-tube by the decomposition of the carbonic-acid gas could readily be converted into ozone, but that it was difficult to pass this limit. This amount corresponds with the formation of a gas constituted of ozone and oxygen in the proportion of two units of ozone to one unit of oxygen, the matter of the oxygen being thus distributed $2\xi^3 + \xi^2$. In several experiments, nevertheless, this limit was considerably exceeded; and a table is given of nine experiments, in which the amount per cent. of ozone ranged from a minimum of 76.6 per cent. to a maximum of 85.5 per cent. This latter amount corresponds with a gas constituted of ozone and oxygen in the proportion of four units of ozone to one of oxygen, thus $4\xi^3 + \xi^2$. In this case the ratio of the total oxygen present to the titre of the gas is 3.5; if the whole oxygen were converted into ozone, the value of this ratio would be 3.

These experiments, taken in connexion with those described in my previous communication, leave no room for reasonable doubt as to the composition of the unit of ozone, which is the triad form of the element oxygen, and is to be symbolized as ξ^3 , an hypothesis henceforth to be regarded as conclusively demonstrated. This remarkable discovery has many important bearings upon chemical theory, especially in relation to the problem elsewhere raised by me of the true nature of the elemental bodies and the constitution of the unit of the element chlorine, which ozone so closely resembles in its chemical properties, and which is also a triad element symbolized as $\alpha\chi^2$.

Nov. 27.—W. Spottiswoode, M.A., Treasurer and Vice-President, in the Chair.

The following communication was read:—

“On the Quantitative Analysis of certain Alloys by means of the Spectroscope.” By J. Norman Lockyer, F.R.S., and William Chandler Roberts.

The authors, after referring to experiments which showed clearly that the spectroscope might be employed to detect minute differences in the composition of certain alloys, proceed to give an account of the researches which they had instituted with a view to ascertain the degree of accuracy of which the method is capable.

The image of an electric spark passing between the unknown alloy and a fixed electrode being thrown by means of a lens on the slit of the spectroscope, the phenomena observed were found to vary with the composition of the alloys; and further, by arranging them together with known check-pieces on a suitable stand, and bringing them in turn under the fixed electrode, the composition of the unknown alloys was determined by comparison with the known check-pieces.

The shape of the electrode ultimately adopted was that represented in the sketch; and these pieces were held in their places by suitable metallic clips. Special attention was then directed to the adjustment of the length of the spark, which was found to materially influence the phenomena. The method adopted consisted in placing the variable electrode in the field of a fixed microscope having a 3- or 4-inch objective, and adjusting the summit of this electrode to coincide with the spider-lines of the eyepiece.



After a series of experiments on alloys of zinc and cadmium of various compositions, the results of which were shown on a curve, more extended trials were made with the gold-copper alloy employed in coinage, which was peculiarly suited to these researches in consequence of the known method of assay having been brought to so high a state of perfection (the composition being determined with accuracy to the $\frac{1}{10,000}$ part of the original assay-piece of about 7 grains), and from the fact that reliance can be placed on its homogeneity. The paper is accompanied by a series of four curves, which show the results of experiments, and in which the coördinates are given by the ordinary method of assay, and by the spectroscopic readings.

The chief practical advantage which appeared to flow from this inquiry was that, if it were possible to replace the parting assay by the spectroscopical method, a great saving of time in ascertaining the value of gold bullion would be effected.

Dec. 18.—Joseph Dalton Hooker, C.B., President, in the Chair.

The following communication was read:—

“On the Period of Hemispherical Excess of Sun-spots, and the 26-day Period of Terrestrial Magnetism.” By J. A. Broun, F.R.S.

It appears from the interesting communication to the Royal

Society, June 19th, by Messrs. De La Rue, Stewart, and Loewy*, that the difference of the area of spots on the visible northern and southern quarter-spheres of the sun seems, during periods of considerable solar disturbance, to obey a law such that the difference is a maximum in the same quarter-sphere during several successive rotations of the sun, the difference being a maximum alternately in the northern and southern hemispheres—the time from maximum to maximum, for the same hemisphere, being variable between 18 and 32 days, but having a mean value of about 25·2 days.

It occurs at once that if the variations of the mean terrestrial magnetic force are connected in any way with the solar spots, or the causes which produce them, we might here find some explanation of the magnetic period of 26 days, the difference of spot-area in one hemisphere from that in the other being related to a difference of the solar magnetic action.

In order to determine whether such a connexion existed, I projected first the curves of excess of spot-area given in the paper of Messrs. De La Rue, Stewart, and Loewy, and below them the daily mean horizontal force of the earth's magnetism during the same periods. The conclusion from these projections is, that there is *no relation whatever between the two classes of curves*. The maxima and minima of the one agree in no way with those of the other: the greatest excesses of sun-spot area in the one hemisphere over those in the other occur when the earth's magnetic force is the most constant; the greatest variations of the earth's magnetic force from the mean occur in several instances when the sun-spot area is equal in the two visible quarter-spheres.

It should be remembered, in considering the curves of sun-spot excess, that the minima and maxima are in some cases only relative, —sometimes the one, sometimes the other being really cases in which there is neither maximum nor minimum—that is to say, cases in which the sun-spot area is equal, or nearly so, in the two visible quarter-spheres.

It would be hasty to conclude from this comparison that the variations of the mean magnetic force are really unconnected with the mode of distribution of the sun-spots. Other methods of grouping the spots may perhaps be employed with advantage relatively to this and other questions; for example, were the position of the centre of gravity of the sun-spots determined for the visible quarter-spheres and hemisphere, giving each spot a weight in proportion to its area, the variation of these positions in latitude and longitude, and their weights, might give a more satisfactory base for this comparison and for other deductions.

It will be obvious also that this investigation refers only to the *visible* hemisphere of the sun; an approximation to the spot-distribution on the other hemisphere, however, will be frequently possible.

* Proc. Roy. Soc. vol. xxi. p. 399.

GEOLOGICAL SOCIETY.

[Continued from p. 235.]

April 30, 1873.—Joseph Prestwich, Esq., F.R.S., Vice-President, in the Chair.

The following communications were read:—

1. "On the Permian Breccias and Boulder-beds of Armagh." By Prof. Edward Hull, F.R.S., F.G.S., Director of the Geological Survey of Ireland.

In this paper the author described certain breccias occurring in the vicinity of Armagh, which he referred, both on stratigraphical and physical grounds, to the Lower Permian series, considering them to be identical with the "brockram" of Cumberland and the breccias of Worcestershire and Shropshire. The lower beds rest on the denuded surface of the Lower Carboniferous Limestone, and consist of a breccia of limestone pebbles in a reddish sandy paste, sometimes becoming a bedded calcareous sandstone with pebbles. These beds, which are 10–12 feet thick, are overlain by soft rudely stratified conglomerate and Boulder-beds of subangular and rounded blocks of grit, felspathic sandstone, vein-quartz, and limestone. In some places the breccia graduates up into the overlying Boulder-beds; but sometimes its surface is eroded. These beds are overlain by Boulder-clay of the Drift period. Within the city of Armagh well-borings &c. have revealed the existence of New Red Sandstone above the Breccia and Boulder-beds. In the author's opinion the only agent which could have brought the blocks of Silurian and Old-Red-Sandstone age found in the Boulder-beds from their place of origin is floating ice. The author further referred to the extensive denudation which the Carboniferous beds have undergone in Armagh, and also alluded to the occurrence of beds of Permian age near Benburb, between Armagh and Dungannon.

2. "Geological Notes on Griqualand West." By G. W. Stow, Esq., F.G.S.

The geological results of a journey made by Mr. G. W. Stow and Mr. F. H. S. Orpen from the Orange Free State into Griqualand West are communicated by Mr. Stow in this paper, with numerous carefully executed sections and a geological map based on the survey map prepared by Mr. Orpen for the Government. From the junction of the Riet and Modder rivers (south of the Panneveldt Diamond-fields) westward to the junction of the Vaal and the Orange, over the Great Campbell Plateau to Griquatown, Ongeluk, Matsáp, Potgieter, the Langeberg, Witte Zand, and to Kheis and the Schurwe Bergen, the track traversed three degrees of longitude, but nearly 300 miles of road. The return route north-east to Mount Huxley and Daniel's Kuil, and eastward to Likatlong, on the Hart or Kolang river, was nearly as long; and its results form part of the present communication. A subsequent portion of the journey up the Hart, across to the Vaal, down the valley by the diamond-diggings of

Hebron and Klipdrift to the Panneveldt, will be treated of in another paper.

From the Modder, first south-westward and then westward, to the junction of the Vaal and Orange, the olive shales of the *Dicynodon* or Karoo series, traversed frequently by igneous rocks, form the country, and are seen in some places to lie unconformably on older rocks. The shales reach to the edge of the Campbell Randt, on the other side of the Orange River, and have been, it seems, to a great extent, formed of the débris of those old hills. The oldest rocks of the locality are seen cropping out here and there in the gorges at the foot of the Randt, and consist of metamorphic rocks, greatly denuded, on which the massive and extensive siliceo-calcareous strata of the Great Campbell Plateau lie unconformably. These latter and the breccias of their slopes are coated thickly with enormous travertine deposits. Beyond the Plateau, at Griquatown, a long parallel range of jaspideous rocks comes out from beneath the Campbell Plateau, presenting a wonderful group of yellow, brown, chocolate, and red jaspers, with magnetic and other ironstone, and beautiful seams of blue and yellow crocidolite. The southern portion of this range has long been known as the "Asbestos Mountains" and the "Doornberg." Igneous rock-masses occur around Ongeluk, west of the Jasper range: and then bright-red jasper rocks crop up near Matsáp, succeeded to the west by the parallel quartzite range of Matsáp, and again by other bedded jaspers, which seem to lie in a synclinal of the quartzite rocks, which come up again in the Langeberg. These are succeeded by lower rocks, consisting largely of sandstone, grit, and quartzite, with more or less pervading mica, as far as the journey extended in the Schurwe Bergen, also parallel to the former ranges. The maximum thickness of the successive strata is calculated by the author at 24,000 feet; allowing for possible reduplications, the minimum is regarded as not less than 9000 feet. The details of stratification, successive upheavals, denudation, nature and origin of the salt-pans, escarpments, river-valleys, and other features were treated of by the author, who has supplied a very large collection of specimens illustrative of the phenomena observed in the line of march, and of his numerous sections, maps, and sketches.

3. "On some Bivalve Entomostraca, chiefly Cypridinidæ, of the Carboniferous formations." By Prof. T. Rupert Jones, F.R.S., F.G.S.

The larger forms of bivalved Entomostraca are not rare in the Carboniferous Limestone; and some occur in certain shales of the Coal-measures. Professors Phillips, M'Coy, and De Koninck have figured and described some specimens. The collections made by M. J. Bosquet, F.C.G.S. (Belgium), Mr. Joseph Wright, F.G.S. (Cork, Ireland), and Mr. J. H. Burrow, M.A. (Settle, Yorkshire), and submitted to the author and his friend Mr. J. W. Kirkby, have elucidated many obscure points in the history of these old Entomostraca. Other specimens from Scotland, Yorkshire, Derbyshire, and the Isle of Man, communicated by many friends, were treated of in this paper.

The *Entomoconchus* of M'Coy proves to have the Cypridinal cha-

racters of anterior sinus and gape; and, besides *E. Scouleri*, at least two species have been determined. A very closely related genus, *Offa*, has also been indicated. M'Coy's "*Daphnia primæva*" belongs to *Cypridina* proper; and twelve other Carboniferous species have been established. A modified form of carapace, without any exact modern analogue, characterizes the new genus *Cypridinella*, with seven species. De Koninck's genus *Cypridella* (restricted) has seven species; and a form intermediate to this and *Cypridina* is described as *Cypridellina*, with eight species. *Sulcuna* (with two species) is an exaggerated modification of *Cypridella*. *Cyprella* of De Koninck (restricted) has two species. The recent *Bradycinetus*, Sars, and *Philomedes*, Lilljeborg, appear to have their prototypes in the Carboniferous Limestone, with one species each. *Rhombina*, a new genus, is a less easily recognized Cypridinal form, with an Irish and a Belgian species.

The recent *Polycope*, Sars, belonging to a different family, and represented by three Carboniferous species, was the last described in the memoir; but *Cytherella*, belonging to an allied group, is known in the Mountain-limestone and Coal-measures; and *Leperditia*, *Entomis*, *Beyrichia*, *Kirkbya*, and other palæozoic genera, abound, together with forms referable probably to *Cythere*, *Cypris*, *Candona*, &c., which will be treated of in a future paper.

XXXVIII. Intelligence and Miscellaneous Articles.

ON THE PERMANENT MAGNETISM OF STEEL. BY E. BOUTY.

THE phrase "coercive force" has ever expressed only a somewhat vague comparison between the phenomena of the magnetization of steel and friction. This bringing together of two orders of phenomena so complex is altogether artificial, and absolutely excludes the facts which relate to the temporary magnetization of steel.

Wiedmann has, more profitably, brought the phenomena in question into comparison with those which depend on elasticity in solid bodies, especially the phenomena produced by torsion. Although this comparison does not constitute a theory properly so called, it has the advantage of expressing a real physical relation; for torsion modifies the magnetic state of a bar, and modifications of that state can in turn modify the torsion to which the bar has previously been submitted. A great number of facts relative to magnetization or demagnetization are conveniently interpreted in this system; but the following phenomena can only with very great difficulty be adapted to it.

It has long been known that a certain temporary magnetism can be superposed to a permanent magnetism opposite in direction, and that the latter may reappear, sometimes even integrally, after the influence of the external forces has ceased. For example, if a bar of magnetized steel be submitted to the action of a current too feeble to demagnetize it entirely, during the action of the current a diminution of the magnetism of the bar is observed, which may go

so far as to the apparent reversal of the poles, whereas after the cessation of the current the bar is found to be still magnetized in the original direction.

The following fact, which I have observed, appears to me still more curious. I take a bundle formed by the combination of four square bars of the same length. It is tempered hard and magnetized immediately; its magnetic moment is measured; lastly it is taken to pieces, and the magnetic moment measured of each bar separately. It is found that the sum of these moments is very considerably greater than the magnetic moment of the bundle. If the bars be united in twos, the sum of the moments of the pairs is intermediate between those of the whole bundle and of the separate bars. When, finally, the bundle is reconstructed, the magnetic moment also returns to its former value.

In this experiment the bundle, which, innocent of all anterior magnetization, has only once undergone the action of the magnetizing spiral, is in an absolutely normal condition at the moment of the first separation; and no new force appears to intervene, to which one might attribute the observed augmentation of the permanent magnetism. It is true that, in separating the bars, we suppress their reciprocal reaction; and we know that in each of them it acted in the opposite direction to that of the permanent magnetism; but this suppression can only have effect on the temporary magnetism. Thus, even in a normal bar, a certain degree of permanent magnetism is found to be superposed to a temporary magnetism of the opposite direction.

It would therefore be very natural to recur to an old hypothesis, according to which the condition, whatever it may be, which corresponds to the conservation of a certain permanent magnetism, is communicated, in the conversion into steel or in the tempering, only to a certain number of molecules, the others retaining their former properties. If we remark (1) that the laws of the temporary magnetism of steel appear to be identical with those of induced magnetism in soft iron, (2) that the development of permanent magnetism is eminently variable from one sort of iron or steel to another, and for one and the same sort according to sometimes insignificant physical conditions, we shall be led to examine more closely than has been done yet the consequences of this hypothesis.

Let us consider a cylinder of elemental dimensions, but of very great length relatively to its diameter. Let us suppose the two kinds of magnetic elements scattered at random, but in a determinate proportion, in all parts of the cylinder, and a magnetic force F acting in the direction of the axis. If the molecules devoid of coercive power existed alone, the cylinder would take a magnetic moment $kF\Delta v$,— Δv representing the volume of the cylinder, and k a coefficient which depends on the density of the molecules. In the same way the molecules endowed with coercive power, if they were alone, would take a magnetic moment $qF\Delta v$.

If we suppose the coefficients of induction k and q constant (which is sensibly true for small values of the inductive forces), and designate by c a coefficient dependent on the grouping of the mag-

netic elements of both kinds, we find that the magnetic moment communicated to the cylinder by the force F will be, taking account of the reactions of the two sorts of molecules.

$$M = \frac{k + q + 2ckq}{1 - c^2kq} F \Delta v ;$$

after the cessation of the force F , they will retain a moment

$$m = q \frac{(1 + ck)^2}{1 - c^2kq} F \Delta v.$$

This quantity is what is ordinarily called the *permanent magnetism*. The temporary magnetism, which disappears on the cessation of the force F , is

$$\mu = M - m = kF \Delta v.$$

It is thus seen that the two coefficients of temporary and permanent magnetism ordinarily determined are not quantities of the same kind. The quantity q , analogous by the part it plays to k , is obtained by dividing the ordinary coefficient of magnetism by $\frac{(1 + ck)^2}{1 - c^2kq}$.

It is evident, and verified without difficulty in a particular case, that the total magnetic moment M is intermediate to those which would be produced by the same force F acting on two cylinders equal to the first, each comprising only one sort of molecules, with the same total density. But it is not the same with the residual moment m , which, for a given value of q , is as much greater as the coefficient k of temporary magnetism itself is greater; and as the coefficient k relative to soft iron is enormous, it is seen that the addition of a certain quantity of soft iron to the hardest steel may augment the residual magnetism of the latter. The employment of armatures of soft iron at the extremities of magnetic bundles, in order to increase their power, comes to the support of our assertion. We will also remark that, according to M. Jamin, the varieties of steel the richest in carbon and tempered the hardest do not present the greatest residual magnetic moments—which is what should be if the molecules of soft iron in them are very rare, as we must suppose them to be. We reserve to ourselves to return to this subject subsequently, and to develop the results by calculation and experiment.

The complete theory of the phenomena of breaking, on which we were engaged in a preceding communication, required a rigorous knowledge of the two functions of temporary and permanent magnetism of steel. Let us consider two bodies, A and B, submitted to one and the same inductive force, but united invariably the one to the other. After the cessation of the inductive force the body A remains under the action of B, and conserves, apart from the residual magnetic moment which would remain to it after its separation from B, a moment (of the same or the contrary direction) produced by the influence of B, and which is permanent only so long as the union between A and B subsists. This magnetic excess might be named the *subpermanent magnetic moment*. In the experiment

of the bundle broken parallel to the axis the *subpermanent* magnetism is contrary in direction to the permanent magnetism. It would be the same in direction in the case of a needle broken in a plane perpendicular to its axis, and of which the two fragments were separated or reunited end to end. This result has been verified by experiment.—*Comptes Rendus de l'Acad. des Sciences*, Feb. 23, 1874.

CONTRIBUTION TO THE THEORY OF THE MAGNETIC FORCES.

BY M. STEFAN.

The memoir consists of three parts. In the first, "on the calculation of the magnetic forces of electric currents," it is pointed out that the equivalence between the forces emanating from magnets and those from electric currents is complete, not only, as is known, in the exterior, but also in the interior space, and that in this the action of a magnet on a point outside of its elements must be different from its action on a point within them. A simple rule is given for the calculation of the electromagnetic forces; and it is specially noted that the interior of a sphere round which currents run in parallel circles presents a homogeneous magnetic field, that the same property belongs to an ellipsoid, and that such systems of currents furnish galvanometer- and magnetizing spirals of constant force.

In the second part, "on the action of a magnet on an internal point," this problem, already touched upon in the first part, is treated more in detail. It is shown that the action of a magnet on an internal point is not completely determined by the magnetic potential—that other forces besides those given by that potential are operative, differing in direction and quantity according as the point affected is within or without a molecule of the magnet. These forces are dependent on the form and arrangement of the molecules, and so constituted that the sum of their works on a finite path is $=0$. Only when the magnetism of the molecules consists of electric currents is the latter generally not the case, and the principle of the conservation of energy requires the entrance of induction currents.

The third part has for its subject the "theory of magnetic induction." The basis of it is formed by the theorem, found in the second part, of the action of a magnet on a point in the interior of its molecules. On the foundation of this theorem the general equations of the theory of magnetic induction and of the theory, identical with it, of *dielectric polarization* can be immediately written, and, with the aid of some theorems found in the first part, some problems on the magnetizing of a sphere, an ellipsoid, a ring, be solved without further calculation. Several series of experiments are then discussed, from which it results that all sorts of iron and steel admit the same maximum of magnetization, that the resistance of iron and nickel to magnetizing is initially very great, then diminishes to a minimum, which is reached when the induced magnetic moment is one third of its maximum, and that thenceforward the resistance again increases till it reaches an infinite value. From these data and some general considerations is deduced a formula for

the magnetic molecular force, which accords well with experiment.
—*Sitzung der math.-naturw. Classe d. kaiserl. Akad. d. Wissensch. in Wien*, February 12, 1874.

ON A PECULIAR OCCURRENCE OF CRYSTALLIZED SILICIC ACID.

BY THEODOR HÜBENER, OF ROSTOCK.

About a year since, I was treating some Gölitze browncoal for the purpose of exhibiting microscopic products with Schultze's maceration-fluid (a solution of chlorate of potash in nitric acid). After the coal had been sufficiently treated with the liquid, and then extracted with ammonia and alcohol, the microscopic examination showed in the residue a quantity of minute crystals the form of which (six-sided columns pointed at both ends) left no doubt that they consisted of crystallized quartz. By repeated ignition of the residue and treatment with hydrochloric acid, they could be readily isolated. From, at the most, 1 kilogramme of the coal, I have in this way obtained about 3 grammes of the crystals. I have not at present found them in other browncoals. Unfortunately I have not yet succeeded in obtaining some of the sand which perhaps overlies the browncoal; but as the residue above mentioned contains scarcely any grains of ordinary quartz, it might be supposed that, by the influence of the products occurring in the gummification of coal (humic acid &c.), solution of siliceous salts infiltrated into it may be decomposed, whereupon the silicic acid is separated as crystals. My attempts (only on a small scale, it is true) to separate silicic acid in crystals by protracted digestion of humous substances with solutions of siliceous salts have been without result. I shall, however, continue these experiments during a longer time; and I have no doubt that in this way I shall succeed in separating crystallized silicic acid from solutions.—Poggendorff's *Annalen*, vol. cl. p. 643.

ON THE SPECTRUM OF THE LIGHT OF EXPLODING GUN-COTTON.

BY O. LOHSE.

The yellowish flame produced when pyroxyline is ignited gives a very interesting spectrum. I first observed it, with a Browning miniature spectroscope, in January 1872, at a time when the frequently occurring phenomena of the aurora borealis induced me to investigate the origin of its spectrum. I thought then, on superficial consideration, that I could recognize a certain similarity between the latter and the spectrum of exploding gun-cotton—a sufficient reason for undertaking, in conjunction with Dr. Vogel, an accurate examination of this spectrum with a larger spectrum-apparatus which permitted the measurement of the lines and the determination of their wave-lengths. The result was that the lines of the two spectra did not coincide.

If I take leave to communicate in the following the details of the investigation, it is in view of the interesting experiments which have taken place in the Arsenal at Woolwich, on the velocity of propagation of the explosion of pyroxyline.

The brightest lines in the spectrum of this nitro-body are the two sodium-lines; they appear, however, not as in a quietly burning alcohol flame saturated with vapour of sodium, but strikingly widened, so that frequently the space between them vanishes. So far as the observations extend, this appears to be dependent on the violence of the explosion. Also the more refrangible sodium-line is always the most widened.

Characteristic of the spectrum are two brilliant lines in the green, which are indistinct on each side, and of which the more refrangible is the more intense. Further, there are two bright broad bands, striking in the red and orange, sharply defined towards the red end, and dying off towards the blue. Besides these lines and bands, we succeeded in measuring a faint streak in the red, another in the green, and a faint but sharp line in the green. Between the bright lines a continuous spectrum was present; but it could not be traced far beyond the green.

The instantaneousness of the explosion of the pyroxyline rendered extremely difficult the fixing upon the lines; yet I believe that a fair degree of certainty may be attributed to the following numbers, as numerous and repeated measurements were effected:—

Wave-length. millionths of a millim.	
644.6	A faint streak.
622.4	} A broad band of light.
610.3	
601.8	
589.5	} Similar, but more faint.
589.5	
588.9	} The same; the very bright sodium-lines.
572.8	
572.8	A faint streak.
568.7	A faint but sharply defined line.
554.8	A very bright, broad but ill-defined line (aurora borealis 557.1).
550.4.	A bright, broad, ill-defined line.

Poggendorff's *Annalen*, 1873, No. 12, p. 641.

DR. J. H. VON MÄDLER.

On the 14th of March, 1874, one of the first of European astronomers, Dr. Mädler, Professor and Director of the Dorpat Observatory, entered into rest. His life had been one of ceaseless and untiring activity, and he was well known for his general astronomical labours; but the work that will carry his name to posterity perhaps more than any other is the *Mappa Selenographica*, bearing his name conjointly with that of Beer. From the announcement of his death sent by his widow, Minna v. Mädler, to his friends, we learn that during the last sixteen months his sufferings were very great from frequent attacks on his brain, to which he ultimately succumbed when about completing his eightieth year. His works were numerous; and we fully enter into the feelings of the widow in desiring, according to her own expression, "a blessing on his works and peace to his ashes."

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[FOURTH SERIES.]

MAY 1874.

XXXIX. *On the Determination of the prime Angle of Incidence and prime Azimuth for the different Fraunhofer's Lines.* By G. QUINCKE*.

IF under the prime angle of incidence H linearly polarized light be reflected from a plane surface, the plane of polarization making the angle α with the plane of reflection, the two components polarized perpendicular and parallel to the plane of incidence will have a difference of phase of a quarter wave-length, and the ratio of their amplitudes is determined by the tangent of the prime azimuth B , by the equation

$$\frac{S}{P} = \frac{\tan B}{\tan \alpha} \cdot \cdot \cdot \cdot \cdot \cdot \cdot \cdot (1)$$

The reflection is named positive when the component polarized parallel to the reflection-plane is accelerated relatively to that polarized perpendicular to that plane, negative when it is retarded. The former case is the more frequent. With metals, positive reflection only has yet been observed.

If the prime angle of incidence and prime azimuth are known, then from these the difference of phase and the ratio of the amplitudes, or the intensity of the components of the reflected light, for any angle of incidence can be calculated by means of formulæ which, although of quite a different form and found in a very different way, represent the phenomena equally well, within the possible errors of observation. (Conf. Pogg. *Ann.* vol. cxxviii. pp. 387-399, 541-564. 1866; vol. cxxxvi. p. 585. 1869.)

* Translated from a separate impression, communicated by the Author.
Phil. Mag. S. 4. Vol. 47. No. 313. May 1874. Y

The prime angle of incidence and azimuth are thus determined:—The difference of phase of the two components polarized parallel and perpendicular to the plane of incidence are, in any way, made equal to a high multiple of $\pm \frac{\lambda}{2}$. The two then give

linearly polarized light, which being extinguished by an analyzing Nicol prism, its plane of polarization can thus be determined.

The light originally polarized linearly, and after reflection elliptically polarized, can be converted into linearly polarized (1) by being again reflected once or more times under the same conditions, (2) by annihilating the difference of phase by a plate of crystal of variable thickness (a plate of calc spar cut perpendicular to the optic axis, a Babinet's compensator) whose principal section coincides with the plane of reflection, (3) by turning the principal section of a crystal plate of invariable thickness (a plate of mica of $\frac{\lambda}{2}$) toward the reflection-plane.

The first method has been used by Brewster* and Jamin†; the second by Brewster*, Jamin‡, Van der Willigen§, Haughton||, and me¶; the third by De Sénarmont**, Kirchhoff, and Eilhard Wiedemann††.

The most perfect process is that adopted by Jamin, who threw a pure (objective) spectrum with Fraunhofer's lines upon a polarizing Nicol in azimuth 45° , and with the analyzing Nicol in azimuth β extinguished the rays twice reflected under the prime angle of incidence H by parallel mirrors. The angle of incidence and azimuth β were altered until the field appeared as dark as possible.

Azimuth B is then given by the equation

$$B = \arccos(\tan \beta / \tan H). \quad \dots \dots (2)$$

Jamin determined in this way the optical constants of several metals for different Fraunhofer lines, and thence deduced the various colours of the metals.

This procedure has many inconveniences, especially this, that the angle of incidence cannot be determined with sufficient accuracy. A slight modification of it, which I have employed for more than ten years, consists essentially in observing the spec-

* Phil. Trans. 1830, pp. 294 & 312.

† *Ann. de Chim.* (3) vol. xxii. p. 311 (1848).

‡ *Ibid.* vol. xxix. p. 281 (1850).

§ *Pogg. Ann.* vol. cxvii. p. 464 (1862).

|| *Phil. Trans.* 1863, p. 122.

¶ *Pogg. Ann.* vol. cxxviii. pp. 355 & 541 (1866).

** *Ann. de Chim.* (2) vol. lxxiii. p. 337 (1840).

†† E. Wiedemann, *Elliptische Polarization des Lichtes*. Leipzig: 1872.

trum with Fraunhofer's lines not objectively, but subjectively, and permits the prime angle of incidence and prime azimuth to be determined with any accuracy desired, as soon as reflecting surfaces generally can be produced of homogeneous quality.

Through a collimator with a vertical slit and an achromatic lens, a pencil of parallel rays falls upon a polarizing prism standing in the azimuth 45° , and then upon a horizontal astronomical auxiliary telescope, the cross-threads of which can be illuminated by a plane glass (between the eyepiece and the cross-threads) inclined 45° to the axis of the telescope.

Across the slit of the collimator runs a thin wire, to which the cross-thread of the telescope is adjusted. It lies on the axis of the collimator when its position is unaltered on turning the latter 180° .

Between the collimator and the telescope is a large smoothed cast-iron frame, on which an iron sledge with a horizontal circle can be moved parallel to itself. A little table in the centre of the circle, and the circle itself, are to be moved each by three adjusting-screws, and the rotation of the table to be measured by two diametral verniers. On the table a plane-parallel glass is fixed, so that the image of the cross-threads of the telescope is reflected towards the latter. When the plane glass is rotated 180° the cross-threads and their image again coincide, if the axis of rotation of the circle is normal to that of the collimator. The normal of the plane glass and the axis of the collimator then coincide; the angle of incidence is 0° .

The azimuth 90° of the polarizing Nicol is determined thus:—The slit of the collimator is illuminated with daylight or with a sodium-flame, the angle of incidence made nearly equal to the angle of polarization, and the glass plate and the polarizing Nicol rotated until the image of the slit, observed with the naked eye or a telescope, shows a minimum intensity of light (0).

The analyzing Nicol, fixed on a vertical circle, stood at 0° when, after the removal of the plane glass, the light issuing from the strongly illuminated slit, and polarized by the Nicol in the azimuth 0° , was completely extinguished.

The vertical circle with the analyzing Nicol could either be moved parallel to itself on a horizontal plane table-plate, or it was fastened in front of the objective of a telescope which was directed to the slit of the collimator and could be moved parallel to itself on a cast-iron sledge with a frame, like the horizontal circle.

Between the eye and the analyzer I brought a flint-glass prism of from 30° to 60° refracting angle with a vertical edge, or a system of prisms for direct vision. The light from the slit of

the collimator gives then a horizontal spectrum with Fraunhofer's lines of from 1° to 3° breadth.

On the table of the horizontal circle, instead of the plane glass, two parallel plane mirrors of the substance to be examined are fastened with their ends overlapping, so that one of them by reflection of the cross-threads can be placed perpendicular to the axis of the auxiliary telescope or the collimator, and its parallelism be tested by turning it through 180° . The mirrors were, by means of the arrangement described in Pogg. *Ann.* vol. cxlii. p. 198, pl. 5. fig. 3, placed parallel, being fastened with wax on the two sledges, the faces pressed against each other, and the sledges drawn asunder.

Frequently they were only fastened with wax upon a horizontal glass plate, and one of the mirrors rotated until the twice reflected image of a distant object (a tree on the horizon) was coincident with the object itself.

If from a heliostat sunlight be let through the slit of the collimator, and polarized in the azimuth $\pm 45^\circ$, to the parallel mirrors, and the light reflected under the prime angle of incidence be taken up by the analyzing Nicol in the azimuth $\mp \beta$, a dark streak appears in the spectrum, which can be made to fall on a determined Fraunhofer line by changing the angle of incidence H and the azimuth β . The reading of the incidence-angle gives directly the prime angle of incidence H for the Fraunhofer line in question, and equation (2) the corresponding prime azimuth.

From the determinations with positive and negative angles of incidence and positive and negative azimuths the mean was taken.

The defects of the method consist in the difficulty of obtaining Nicol prisms with even surfaces, which, brought in front of the objective of the collimator and the telescope, would give distinct images of the slit, and, further, that the incident and the issuing ray make with each other an angle which may amount to as much as 1° . For this reason I have mostly omitted the observing telescope between the eye and the analyzer.

On turning the polarizing Nicol from the azimuth $+45^\circ$ to -45° the image of the collimator-slit was displaced towards the cross-thread of the auxiliary telescope $-8'$, as shown by the image of the thread reflected by the plane glass. This alteration of the angle of incidence $-8'$ had to be taken into account in the determination of H .

The divisions of the circle were read with verniers accurately to $1'$. The error of a single determination of H or β scarcely amounts to $1'$, if the reflecting surface is even and homogeneous and has at all parts the same optical constants H and B .

Variations of temperature, however, and pressure in polishing

frequently alter the optical qualities of a mirror very considerably; and it is probably much more difficult to obtain faultless reflecting surfaces than transparent substances free from veins. Besides these irregularities, disturbance is occasioned by unavoidable scratches and roughnesses of the surface, which act like a diffraction-grating. As in the case of coefficients of elasticity or electric conducting-power, properly the value of H and B must be determined for each definite portion.

The following Table (p. 326) contains a series of determinations of the prime angle of incidence and prime azimuth for Fraunhofer's lines C, D, E, F, and G, executed by the method of parallel mirrors; and the numbers in general, so far as they relate to substances previously investigated, agree with the above-mentioned measurements by Jamin and Haughton.

The gold was burned in upon plate glass by heating a solution with essential oil; copper and nickel were electro-deposited; the cobalt consisted of polished pieces such as occur in commerce. Antimony, bismuth, zinc, and tin were cast and polished plates; aluminium, brass, and platinum were rolled; the silver was obtained by Martin's process (*Pogg. Ann.* vol. cxxix. 1866, p. 55) from a solution several months old.

For reflection in glass, the silver was fixed on the long, parallel sides of two Fresnel parallelepipeds, which had an acute angle of $56^{\circ} 52'$ and the exponents of refraction

C.	D.	E.	F.	G.
1.5220	1.5246	1.5279	1.5308	1.5362

In the calculation of the prime azimuths, account is taken of the rotation of the plane of polarization on the entrance into and the issue from the glass. To the selenium a reflecting surface had been imparted by pressing cold plates of glass on the melted substance.

The most perfect surfaces were possessed by nickel, silver, gold, and selenium.

Where not stated otherwise, the reflection took place in air; and shortly before the observation the metals had been polished with clear buckskin and a trace of jeweller's rouge.

With the exception of gold, in the case of all the metals here mentioned the prime angle of incidence (polarization-angle) diminishes with the diminution of the wave-length—the reverse of what takes place with transparent bodies. The diminution, however, is very different with the different metals.

The prime azimuths partly increase, and partly diminish as the wave-lengths become less. Platinum shows a maximum value for the line D, cobalt and bismuth for the line E, and tin for the line F.

TABLE I.

No.	Substance.	Prime Angle of Incidence, H.					Prime Azimuth, B.					H _C - H _G , B _C - B _G .	
		C.	D.	E.	F.	G.	C.	D.	E.	F.	G.		
1.	Aluminium	77° 10'	76° 16'	75° 14'	74° 12'	72° 48'	35° 9'	35° 54'	36° 36'	37° 26'	38° 1'	4° 22'	2° 52'
2.	Antimony	79° 44'	79° 12'	78° 6'	76° 56'	74° 23'	28° 4'	31° 7'	33° 23'	35° 3'	35° 36'	5° 21'	7° 32'
3.	Cobalt.....	76° 45'	76° 12'	75° 8'	74° 15'	71° 52'	30° 59'	30° 45'	31° 56'	31° 33'	32° 28'	4° 53'	1° 29'
4.	Gold	72° 47'	70° 38'	66° 32'	63° 21'	65° 8'	43° 12'	42° 35'	40° 22'	33° 5'	28° 23'	7° 38'	14° 49'
5.	Copper	71° 21'	69° 9'	66° 3'	63° 19'	62° 20'	40° 1'	40° 25'	39° 3'	36° 30'	29° 54'	9° 1'	10° 7'
6.	Brass	74° 19'	71° 41'	68° 59'	66° 50'	63° 11'	41° 2'	40° 52'	39° 7'	36° 26'	29° 46'	11° 16'	11° 16'
7.	Nickel	77° 22'	76° 7'	74° 55'	74° 4'	72° 5'	32° 12'	32° 9'	32° 2'	32° 0'	31° 39'	5° 17'	0° 33'
8.	Platinum	78° 28'	77° 8'	76° 8'	75° 21'	73° 39'	31° 58'	32° 46'	32° 28'	32° 26'	32° 17'	4° 49'	0° 19'
9.	Selenium	71° 26'	71° 23'	71° 10'	71° 0'	70° 10'	8° 24'	11° 32'	12° 13'	13° 14'	15° 30'	1° 16'	7° 6'
10.	Silver, unpolished	72° 30'	70° 42'	68° 40'	67° 18'	64° 13'	43° 34'	42° 45'	42° 50'	42° 20'	41° 16'	8° 17'	1° 18'
11.	" polished	74° 5'	72° 10'	70° 22'	68° 44'	66° 14'	41° 53'	41° 40'	41° 10'	40° 35'	40° 32'	7° 51'	1° 41'
	" unpolished, in glass ..	71° 17'	68° 55'	66° 58'	65° 16'	62° 38'	42° 37'	42° 17'	41° 44'	41° 26'	40° 47'	8° 39'	1° 50'
12.	" polished, in glass ..	73° 17'	69° 51'	67° 40'	65° 46'	62° 45'	42° 45'	42° 49'	42° 43'	42° 18'	41° 55'	10° 32'	0° 50'
	Silver-collodion	67° 17'	66° 16'	65° 15'	64° 7'	62° 11'	20° 21'	21° 12'	22° 26'	22° 0'	25° 12'	5° 6'	4° 51'
13.	Silver in glass ..	60° 11'	58° 9'	56° 54'	55° 19'	53° 51'	20° 56'	26° 11'	28° 53'	32° 23'	33° 4'	6° 20'	12° 8'
14.	Steel	76° 20'	75° 28'	74° 46'	73° 56'	72° 29'	27° 46'	28° 16'	28° 56'	29° 36'	29° 57'	3° 51'	2° 11'
15.	Bismuth	77° 9'	75° 14'	73° 47'	72° 41'	70° 58'	34° 22'	34° 30'	34° 46'	34° 41'	34° 40'	6° 51'	0° 18'
16.	Zinc, cast	80° 29'	79° 34'	77° 58'	76° 54'	74° 51'	31° 20'	35° 5'	37° 26'	38° 20'	39° 5'	5° 38'	7° 45'
17.	" rolled	81° 12'	78° 57'	77° 39'	76° 28'	74° 14'	35° 19'	35° 57'	36° 3'	39° 17'	40° 20'	6° 58'	5° 1'
18.	Tin	77° 16'	75° 40'	73° 23'	72° 40'	70° 8'	34° 34'	35° 29'	36° 2'	36° 15'	36° 11'	7° 8'	1° 37'

Temperature 18-23° C.

According to my earlier investigations (Pogg. *Ann.* vol. cxxviii. p. 562, vol. cxxix. p. 211, 1866), a prime angle of incidence and a prime azimuth can still be spoken of when the light is reflected in *air* from a thin, transparent lamina of metal lying on a glass plate. The difference of phase and ratio of amplitudes of the components polarized parallel and perpendicular to the plane of reflection can thence be calculated for any angle of incidence, just as with non-transparent metallic mirrors.

I have therefore determined the prime angle of incidence and prime azimuth on transparent laminæ of gold, platinum, and silver, as well as on non-transparent metals. (See Table II.) They lay on plates of mirror-glass, the back of which was blackened with oil-colour in order to destroy their reflection.

The gold was obtained by Wernicke's process (Pogg. *Ann.* vol. cxxiii. p. 183, 1868). The silver was obtained thus: from the same freshly prepared Martin's silvering-fluid, on strips of the same plate glass, dressed simultaneously in the same manner, I let silver be precipitated during $\frac{1}{2}$, 2, 3, 6, and 15 minutes respectively. The thicker layers were converted into silver iodide; and from the colour of the iodide layer the thickness of the silver was calculated (cf. Pogg. *Ann.* vol. cxxix. p. 208, 1866). The platinum was a mirror, such as have been sold in France for some years past. The thickness given of the gold and platinum layers is only from a very rough estimate.

The observations on the thinnest layer of silver, as it was not everywhere of equal thickness, deserve but little confidence. The rest of the mirrors of silver, however, were most perfect and homogeneous, and reflected the light strongly, although they had merely been rinsed with water and dried but not polished with leather.

The polarization-angles of the unsilvered glass plates were calculated from the refraction-exponents of the surface, determined by a peculiar process. The glass for the gold layers had exactly, and that for the platinum very nearly, the same exponents of refraction as for silver.

These measurements agree substantially with the results of the previous investigations, which yet were made with only one-coloured red light. They show that the prime angle of incidence and the prime azimuth increase as the thickness of the metal increases, but in different degrees for different colours. The values of the prime azimuths for the different Fraunhofer lines approximate to one another with increasing thickness of silver, recede from one another with greater thickness of gold.

With silver the prime azimuth exhibits a maximum value for a certain Fraunhofer line, which maximum moves towards the red end of the spectrum as the thickness is augmented.

TABLE II.

No.	Thickness of the metal.	Prime Angle of Incidence, H.					Prime Azimuth, B.					H _C -H _G .	B _C -B _G .
		C.	D.	E.	F.	G.	C.	D.	E.	F.	G.		
1.	Gold. millim. 0.000 030 (?)	67 15'	62 1'	56 34'	56 26'	56 48'	28 25'	29 29'	26 21'	21 42'	19 28'	10 27'	8 57'
2.	0.000 045 (?)	71 51	69 0	64 33	62 18	64 27	40 31	40 36	37 54	31 34	28 40	7 24	11 51
3.	Not transparent.	72 47	70 38	66 32	63 21	65 8	43 12	42 35	40 22	33 5	28 23	7 39	14 49
4.	Platinum. 0.000 040 (?)	75 45	74 47	73 51	72 33	71 47	23 33	25 28	26 23	26 26	26 39	3 58	-3 3
5.	Not transparent.	78 28	77 8	76 8	75 21	73 39	31 58	32 46	32 28	32 26	32 17	4 49	-0 19
6.	Silver. 0	56 32	56 33	56 37	56 40	56 47							
7.	0.000 008	57 59	60 31	60 28	59 57	58 45	3 14	2 23	6 57	5 35	5 46	-0 46 (?)	-2 32 (?)
8.	0.000 031	73 52	71 58	70 2	68 27	65 5	32 59	34 21	35 16	36 28	36 49	8 47	-3 50
9.	0.000 046	74 9	71 58	70 21	68 40	65 42	40 26	40 2	40 25	40 40	40 38	8 27	-0 12
10.	0.000 068	75 0	73 23	71 9	69 24	66 18	43 13	43 45	43 51	43 35	43 35	8 42	-0 22
11.	0.000 075	75 39	73 49	71 45	70 0	67 9	43 35	43 54	43 49	43 42	43 19	8 30	+0 16
"	The same, polished.	77 9	75 31	73 39	72 6	69 29	44 14	43 50	43 49	43 47	43 41	7 40	0 33

Gold, in transparent as in non-transparent layers, has a minimum angle of incidence for Fraunhofer's line F.

The values of H and B were not altered by magnetizing or electrizing the metallic mirrors.

By polishing and pressure of the layer of metal the value of the prime angle of incidence is augmented, that of the prime azimuth diminished. I have observed this also with other metals than those cited in Tables I. and II., for example cobalt and nickel. The treatment may vary much for the different colours.

Only with reflection from silver into glass was there shown a slight increase of the prime azimuth through pressure, which, however, was not exerted in this case on the reflecting front surface of the metal, but on its hinder surface bounded by the air.

The effect of pressure, or of the distance of the particles of silver, upon the constants of reflection was most strikingly exhibited with silver-collodion films, for which I am indebted to M. F. A. Nobert, of Barth. They were obtained by the process used in photography—a glass plate coated with collodion (containing iodide of potassium?) being immersed in a silver solution, browned by exposure to daylight, and made black by a strong solution of pyrogallie acid. After drying, the plate was strongly heated, in order to make the collodion film adhere more firmly to the glass.

Such a collodion film contains uniformly distributed finely divided particles of silver, is opaque even when very thin, but reflects too little light for the double-reflection method. With Babinet's compensator and homogeneous red or blue glass, trial was made with reflection in air:—

	Red.	Blue.
Prime angle of incidence .	50° 29'	50° 0'
Prime azimuth	21 20	20 44

After gentle rubbing with soft buckskin, when the plate took a polish:—

	Red.	Blue.
For reflection in air,		
Prime angle of incidence .	64° 50'	62° 2'
Prime azimuth	11 26	13 28
For reflection in glass,		
Prime angle of incidence .	57 25	54 41
Prime azimuth	25 7	41 13

An ordinarily transparent collodion film, rendered opaque by finely divided particles of silver, shows therefore a slight alteration of the prime angle of incidence, but a great alteration of the prime azimuth, which approximates to that of pure silver.

If by a slight pressure the silver particles are brought nearer

to one another, the prime angle of incidence and prime azimuth change very considerably in the same direction, only more than with a layer of pure silver particles on glass. Moreover, with a suitable pressure and reflection in air the prime azimuth may be nearly the same for blue as for red.

Whether the silver-collodion film be unpolished or polished, the particles of silver have always a different distance from one another, or a different distribution, in the vicinity of the glass-surface and in the vicinity of the free surface bounded by air; so that the reflections in air and in glass are not directly comparable.

According to the distribution of the silver particles in the interior of the collodion, which varies of course with the nature of the exposure in its preparation, the prime angle of incidence and prime azimuth vary for different colours, and the silver collodion-film (photographic plate) exhibits various colours under the incidence of natural white light, which may be different for reflection in air and in glass.

Precisely similar is the behaviour of substances with so-called surface-colours, with which H and B increase and diminish with increasing wave-length.

Carthamin, dried in a red-coloured transparent layer on the faces of the above-mentioned parallelepipeds, exhibited, with reflection in the same substance, different reflection-constants in different parts, according as the distance between the particles was less or greater. The parallel-mirror method showed, for light of the given wave-lengths λ (in millionth parts of a millimetre), the following mean values of the prime angle of incidence H and prime azimuth B:—

Reflection in Air.

λ .	H.	B.
656 . . .	59° 2'	4° 52'
589 . . .	64 30	8 56
555 . . .	63 18	14 24
527 . . .	60 27	21 58

Reflection in Glass.

656 . . .	55 12	8 56
589 . . .	59 2	7 24
555 . . .	57 39	14 25
527 . . .	63 48	10 32
486 . . .	61 46	15 32
460 . . .	61 48	24 2
431 . . .	61 14	26 41

It cannot be surprising that the values of the reflection-constants change with the distance of the opaque particles, since the transparency and colour of thin layers of the same metal also depend on it. (Faraday, Exp. Res. iv. 391.)

The dependence of the optical properties of a quantity of more or less transparent and non-transparent particles on the distance of the latter seems to me hardly compatible with the assumption that absorption and dispersion of light are determined solely by a mutual vibration of the molecules of the body, occasioned by the motion of the æther.

Würzburg, Aug. 20, 1873.

XL. On Sylvester's and other forms of continued Fraction for Circle-quadrature. By THOMAS MUIR, M.A., F.R.S.E., Assistant Professor of Mathematics in Glasgow University.*

IN the Philosophical Magazine for May 1869, Professor Sylvester gave as a deduction from a solution of an equation in finite differences the following notable identity:—

$$\frac{\pi}{2} = 1 + \frac{1}{1 + \frac{1 \cdot 2}{1} + \frac{2 \cdot 3}{1} + \frac{3 \cdot 4}{1} + \dots}$$

The origin of it was so peculiar, that to the reader it could not but seem a desirable thing to establish a more general result, and point out some connexion between it and previous special results of the same kind. This has been done in one direction by Mr. J. W. L. Glaisher, in a paper recently read before the Mathematical Society of London, in which he transforms the product

$$\left(1 + \frac{a_1}{b_1}\right) \left(1 + \frac{a_2}{b_2}\right) \left(1 + \frac{a_3}{b_3}\right) \dots$$

into a series, this series into a continued fraction, and then points out that, from the identity of the product and the continued fraction, Professor Sylvester's result directly follows as a particular case. The object of the present short paper is to establish another such line of relationship.

Consider the definite integral $\int_0^1 x^n \frac{(x-1)^{m-1}}{(x+1)^{m+1}} dx$. By integration by parts we find that

$$n \int_0^1 x^{n-1} \left(\frac{x-1}{x+1}\right)^m dx = -2m \int_0^1 x^n \frac{(x-1)^{m-1}}{(x+1)^{m+1}} dx;$$

* Communicated by the Author.

and the left-hand member being also evidently

$$= n \int_0^1 x^{n+1} \frac{(x-1)^{m-1}}{(x+1)^{m+1}} dx \cdot n \int_0^1 x^{n-1} \frac{(x-1)^{m-1}}{(x+1)^{m+1}} dx,$$

we therefore have

$$\begin{aligned} n \int_0^1 x^{n-1} \frac{(x-1)^{m-1}}{(x+1)^{m+1}} dx \\ = 2m \int_0^1 x^n \frac{(x-1)^{m-1}}{(x+1)^{m+1}} dx + n \int_0^1 x^{n+1} \frac{(x-1)^{m-1}}{(x+1)^{m+1}} dx; \end{aligned}$$

and hence, writing $\phi(n)$ for $\int_0^1 x^{n-1} \frac{(x-1)^{m-1}}{(x+1)^{m+1}} dx$, there results

$$n \frac{\phi(n)}{\phi(n+1)} = 2m + \frac{n(n+1)}{(n+1) \frac{\phi(n+1)}{\phi(n+2)}}.$$

Now the denominator on the right is the same function of $n+1$ that the left-hand member is of n ; and thus by continued substitution we have

$$n \frac{\phi(n)}{\phi(n+1)} = 2m + \frac{n(n+1)}{2m} + \frac{(n+1)(n+2)}{2m} + \frac{(n+2)(n+3)}{2m} + \dots$$

If n be now taken equal to 1, the left-hand member here becomes

$$\int_0^1 \frac{(x-1)^{m-1}}{(x+1)^{m+1}} dx \div \int_0^1 x \frac{(x-1)^{m-1}}{(x+1)^{m+1}} dx;$$

or, after integration and substitution of y for $\frac{1-x}{1+x}$,

$$\frac{1}{2m \int_0^1 y^{m-1} (1+y)^{-1} dy - 1};$$

and thus we have

$$2m \int_0^1 y^{m-1} (1+y)^{-1} dy = 1 + \frac{1}{2m} + \frac{1 \cdot 2}{2m} + \frac{2 \cdot 3}{2m} + \frac{3 \cdot 4}{2m} + \dots \quad (\text{I.})$$

Putting in this general result $m = \frac{1}{2}$, we obtain, as desired,

$$\frac{\pi}{2} = 1 + \frac{1}{1} + \frac{1 \cdot 2}{1} + \frac{2 \cdot 3}{1} + \frac{3 \cdot 4}{1} + \dots \quad (\alpha)$$

and putting $m=1$,

$$2 \log 2 = 1 + \frac{1}{2} + \frac{1 \cdot 2}{2} + \frac{2 \cdot 3}{2} + \frac{3 \cdot 4}{2} + \dots \quad (a)$$

and so on.

But, secondly, by expanding $y^{m-1}(1+y)^{-1}$ in a series of ascending powers of y , and integrating with respect to y between the limits 0 and 1, we obtain

$$2m \int_0^1 y^{m-1}(1+y)^{-1} dy = 2 \left(1 - \frac{m}{m+1} + \frac{m}{m+2} - \frac{m}{m+3} + \dots \right).$$

Now the series here enclosed in brackets may be looked on as a particular case (viz. $\alpha=m$, $\beta=1$, $\gamma=m+1$, $x=-1$) of Gauss's general series $F(\alpha, \beta, \gamma, x)$, which, owing to a special property, he was able to expand in the form of a continued fraction. Taking advantage of this, we find, on making the necessary substitutions,

$$\begin{aligned} 2m \int_0^1 y^{m-1}(1+y)^{-1} dy \\ = \frac{2}{1} + \frac{m}{m+1} + \frac{1^2}{m+2} + \frac{(m+1)^2}{m+3} + \frac{2^2}{m+4} + \frac{(m+2)^2}{m+5} + \dots \end{aligned} \quad (\text{II.})$$

and, as before, putting $m=\frac{1}{2}$, we derive the well-known result,

$$\frac{\pi}{2} = \frac{2}{1} + \frac{1^2}{3} + \frac{2^2}{5} + \frac{3^2}{7} + \dots \quad (\beta)$$

and putting $m=1$, there results a companion identity to (a), and so on.

Thirdly, the series found above may be expanded in the form of a continued fraction by means of the ordinary general method. Doing this, it is found that

$$2m \int_0^1 y^{m-1}(1+y)^{-1} dy = \frac{2}{1} + \frac{m}{1} + \frac{(m+1)^2}{1} + \frac{(m+2)^2}{1} + \dots \quad (\text{III.})$$

whence, by the substitution of $\frac{1}{2}$ for m , we obtain

$$\frac{\pi}{2} = \frac{2}{1} + \frac{1^2}{2} + \frac{3^2}{2} + \frac{5^2}{2} + \dots \quad (\gamma)$$

which takes us back to Brounker and the infancy of continued fractions.

The University, Glasgow,
March 28, 1874.

P.S.—Since the above was written I have read part of an encyclopædic paper by Stern in Crelle's *Journal*, vols. x. and xi. (1832–33), entitled “Theorie der Kettenbrüche und ihre Anwendung,” and have to-day arrived with surprise at the third chapter, the first division of which has the same title as Mr. Glaisher's paper above referred to, viz. “Verwandlung der unendlichen Producte in Kettenbrüche.” In this Stern establishes a general formula of transformation, and then proceeds to give examples illustrative of it. Among these are five forms of con-

tinued fraction for $\frac{\pi}{2}$, and first of the five the very fraction we have been speaking of as Sylvester's. Stranger still, in a footnote Stern adds, “Diesen Ausdruck hat schon Euler auf anderem Wege gefunden” (*Com. Ac. Petr.* vol. xi. p. 48); so that the expression derived so ingeniously in 1869 must at that time have been over a century old. Professor Sylvester and Mr. Glaisher will, I am sure, rejoice with other workers to see justice done to their predecessors Euler and Stern.

The University, Glasgow,
April 10, 1874.

XLI. On the Physical Constants of Hydrogenium.

By JAMES DEWAR*.

IN March 1869 I communicated to the Society a paper entitled “Motion of a Palladium Plate during the Formation of Graham's Hydrogenium,” which appears in the Proceedings for Session 1868–69†. When engaged with this subject, many points of interest regarding the behaviour of palladium containing occluded hydrogen suggested themselves for investigation; and in concluding the paper I remarked that “careful determinations must be made of the electromotive force, latent heat, &c.

* From the Proceedings of the Royal Society of Edinburgh, Jan. 20, 1873. Communicated by the Author.

† Phil. Mag. June 1869, pp. 424–431.

of hydrogenium" before we could arrive at any conclusion regarding the condition of the absorbed hydrogen. Subsequently Professor Tait made a series of determinations on the "Electrolytic Polarization of Palladium Electrodes"*, devising a new and ingenious method for the purpose. Although at different times subsequent to my first communication the problem of determining the physical constants of hydrogenium recurred, as my attention was in the meantime directed to the specific heat of carbon at high temperatures, no progress was made with the investigation until September 1872, when the results of my preliminary experiments were communicated to the Philosophical Magazine, under the title "Note on the Specific Heat of Hydrogenium." In that note it is stated that by means of a specially constructed calorimeter the specific heat of hydrogen in palladium is found to be 3.1 per atomic weight, nearly identical with that of gaseous hydrogen. The present paper deals with some of the physical constants of hydrogenium, more especially with the specific gravity, specific heat, and coefficient of expansion.

Graham, in his celebrated paper on hydrogenium, made many determinations of the specific gravity of the occluded hydrogen by observing the increase of length of palladium wire after being fully charged, thus finding the cubical expansion, and from it deducing the weight of unit volume of the absorbed hydrogen.

From experiments made in this way he found the specific gravity to be nearly 2. Afterwards he discovered the value was about three times what it ought to be, from a contraction of length occurring when palladium wire is used. This he confirmed by the use of alloys of palladium that resist this contraction, and finally regarded the specific gravity as 0.733. No determinations were made when the palladium was partially saturated; and he rejected the ordinary process of taking specific gravities, because of the continual evolution of gas preventing exact weighings being taken in water.

In the experiments to be detailed a cubical mass of the metal was charged with hydrogen by electrolysis, taken at different times during the progress of the saturation and weighed in air and in water. If the mass was allowed free exposure to the air for several hours, little difficulty arose from the evolution of gas when immersed in water, and accurate results could be obtained.

From the experimental numbers the specific gravity of the absorbed hydrogen was calculated by the well-known formula

$$\frac{w_1 + w_2}{\frac{w_1}{S_1} + \frac{w_2}{S_2}} = S;$$

* Proceedings of the Royal Society of Edinburgh, Session 1868-69.

where w_1 and w_2 are the weights of the substance, S_1 and S_2 the specific gravities, and S the mean specific gravity found by experiment. This formula assumes that no condensation takes place in the palladium.

TABLE I.—Specific Gravity of Bar Palladium at different times during Saturation with Hydrogen. Weight of original Palladium 31·802.

Experiments.	Weight of palladium and hydrogen in air.	Weight of palladium and hydrogen in water.	Specific gravity of substance.	Specific gravity of hydrogen (calculated).	Weight of hydrogen.
1.	31·8748	29·1150	11·5488	0·6215	0·0728
2.	31·9230	29·0860	11·2520	0·6230	0·1210
3.	31·9425	29·0715	11·1259	0·6150	0·1405
4.	31·9715	29·0615	10·9867	0·6081	0·1625
5.	31·9860	29·0500	10·8944	0·6270	0·1840
6.	31·9955	29·0455	10·8459	0·6299	0·1935
7.	32·0040	29·0450	10·8158	0·6388	0·2020
8 (after some days).	31·9940	29·0325	10·8033	0·6024	0·1920

When the calculated values of the specific gravity of the hydrogen in the eight determinations of Table I. are examined, it will be observed no material alteration can be said to take place in the specific gravity during the course of the saturation, regard being paid to the difficulty of getting accurate weighings immediately after removal from the electrolytic cell. All the above determinations were made as quickly as possible after the palladium was taken from the pole of the battery. The mean of the results gives a specific gravity of 0·620 for the hydrogen. The atomic volume of this condition of hydrogen is therefore 1·6; and the alteration of volume is equivalent to saying that, in the formation of this condition, 7 litres of gas are condensed into 1 cubic centimetre. The maximum charge of hydrogen the palladium would take was given to No. 7 experiment; and this very nearly accords with the composition Pd^3H^2 . In treating masses of palladium the above proportion was never exceeded.

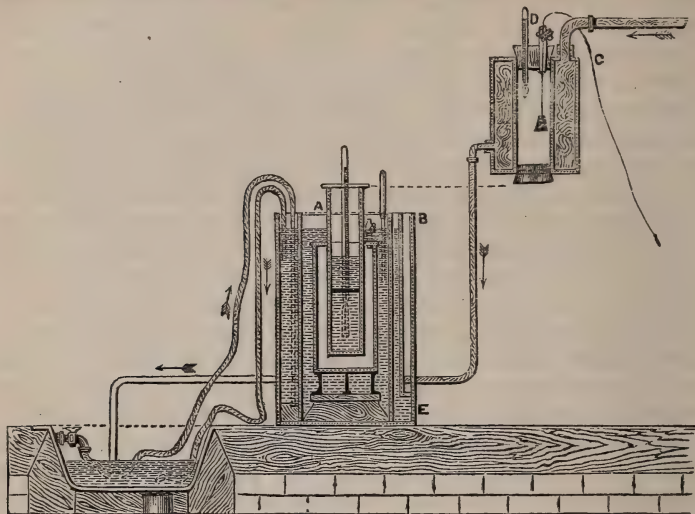
As all the above experiments were made with bar palladium increasing to saturation, it was necessary to find similar values for a mass decreasing from saturation, the hydrogen being expelled by heating. For this purpose a piece of plate palladium was employed. When the plate was fully charged for the first time, and the specific gravity of the hydrogen found as in the previous experiment, the result was a density of 0·621: the plate in this case contained 0·21 grm. of hydrogen. When a portion of the hydrogen was expelled by heat, leaving 0·127 grm.

in the plate, the density of the hydrogen was 0·633; and where only 0·0495 grm. was left, the value was 0·615. The mean specific gravity of the three different alloys of plate was 0·623, nearly the same as previously found for the bar. After palladium has been treated with hydrogen and heated, the metal becomes porous, and often blisters, thus rendering specific-gravity determinations very uncertain. The curious discovery of Graham, that the strain on the particles of palladium produced by wire-drawing induces such a curious shrinking in the length after the hydrogen has been expelled, suggests the importance of investigating how a saturated piece of bar would resist the action of sustained pressure or tension. If the formation of the alloy is attended with an increase of volume in excess of the sum of the volumes of the constituents regarded as in the solid state, then partial decomposition ought to occur under great pressure. Experiments on this subject are left over for the present. After palladium has been used in the above experiments, and the last trace of hydrogen is removed by heating, the specific gravity is found to have diminished from 12·0359 to 11·9546. If this final value is taken in calculating the mean density, then the average result of the three hydrides of plate is 0·707 for the specific gravity of the condensed hydrogen. The mean of the first and second series of experiments is thus 0·664. A piece of palladium, weighing 31 grms., was fused in a lime crucible with the oxyhydrogen-flame; and the specific gravity was now found reduced to 10·5549. When this sphere of palladium was hydrogenized for a very long time (twenty-four hours) only 0·0684 grm. of hydrogen was absorbed, and by further treatment nothing was added. The specific gravity of the hydrogen in this case was 0·655, nearly identical with the mean of the values found from the plate experiments.

Specific-Heat Observations.

The apparatus devised for this purpose is represented in the figure (p. 338); the calorimeter used in the experiments had the form A. It held conveniently 100 grammes of water, and was inserted in the middle of a stout brass envelope, thoroughly exhausted of air, the whole being placed in the middle of a large cylindrical tin vessel (E), having an outer annular compartment. The tin vessel was filled with water; and a constant current from the town supply was kept circulating by means of a siphon through the outer chamber. To the thermometer a ring of thin sheet india rubber was attached as a stirrer, and, when it was removed before the immersion of the palladium, was placed in the little tin tube represented at B. Immediately before an observation the whole apparatus was moved below the steam-

bath (C), and the palladium dropped in. This arrangement of the calorimeter is very convenient in a small chemical labora-



tory, where uniformity of temperature cannot be easily commanded. The constancy of the radiation in the calorimeter makes the correction for cooling very exact.

The same arrangement of the calorimeter is employed for registering very small amounts of heat by placing in A bisulphide of carbon or chloroform, packing the middle and outer compartments of the tin vessel full of pounded ice, and covering the exposed surface with sawdust. The thermometer is now provided with a thin sheet-copper stirrer, instead of the india-rubber one formerly used. The rate of cooling is in this case determined once for all for a range of 5° above zero, and plotted in a curve. From this curve the correction for the radiation is determined for all subsequent experiments.

Two series of experiments were made. In the first series bar, and in the second plate palladium was used, three different hydrides* of each. The experimental results are given in Tables II. and III.

In the case of the bar the specific heat of the occluded hydrogen increased as the charge diminished, the extreme values being 3.79 and 5.05. Similarly with the plate the values are greater, and have a wider range, viz. from 3.93 to 5.88. These results are calculated in each case for the heat given to the

* In this paper the term hydride is not used in its strict chemical sense, but as a convenient abbreviation.

calorimeter by the hydride in excess of that of the original palladium. But if a comparison is made between the different hydrides in both series of experiments, then the specific heat is found to exhibit no such regular increase as in the former series. In the bar the extreme values are 3.21 and 3.77, and the mean of the three results is 3.47. The plate, on the other hand, gives a range of from 2.7 to 3.94, the mean value being 3.31. The amount of variation in the plate is very great as compared with the bar, and is clearly due to some secondary action taking place.

The increase of specific heat for small charges of hydrogen, when comparison is made with pure palladium, is clearly due to some regular increase of the specific heat of the palladium, or of the hydrogen, or of both individually or conjointly. The mere fact that we have only a very small weight of hydrogen relatively to the amount of palladium does not explain the anomaly, because a small amount of hydrogen by the second mode of comparison does not yield such high results. The observational errors, although much greater when we are dealing with small quantities, cannot be expected to fall always in the same direction; and thus we are forced to admit there is some regular sequential change taking place in the relations of the hydrogen and palladium.

The palladium, after use, is not found to have increased in specific heat, but rather the reverse; so that the explanation can only rest on some altered condition of the bodies when united. So far as my experiments have led me, I am inclined to regard this alteration as a kind of molecular dissociation that increases with diminished charge of hydrogen. I am led to this conclusion from observing that the rate at which palladium loses hydrogen at constant temperature is dependent on the amount present increasing with diminished charge. In conducting these specific-heat experiments, a greater variation in the results has always been observed when small charges were under experiment, especially in the first two or three determinations. This increase of dissociation may be explained in part from the formation of regular cracks or channels in the mass of palladium, together with the effect of pressure resulting from the contraction of the external layers of palladium from the loss of hydrogen. It is, however, premature to discuss the cause of this increase of dissociation until a more extended series of experiments is made, and the behaviour of the alloy to pressure is investigated.

TABLE II.—Specific Heat. Experiments with Bar Palladium. Three Hydrides. Weight of Bar 31·802 grms. Water and Calorimeter 107 grms.

	I.	II.	III.	IV.
Weight of hydrogen in pal- ladium	0·1905	0·103	0·0415	Nothing.
Rise in calorimeter for a fall of 100° C.	2·426	2·178	1·944	1·740
	2·430	2·175	1·958	1·780
	2·429	2·175	1·958	1·760
	2·435	2·167	1·967	1·760
Mean rise	2·436*	2·173	1·956	1·760
Rise due to hydrogen	0·676	0·413	0·196	Nothing.
Specific heat of hydrogen	3·79	4·29	5·05	

Weight of hydrogen in	Increase in calori- meter due to dif- ference of	Specific heat from difference of	Mean.
I. in excess of II. ... 0·0875	I. and II. 0·263	I. and II. ... 3·21	} 3·47
II. in excess of III. ... 0·0615	II. and III. 0·217	II. and III. 3·77	
I. in excess of III. ... 0·1490	I. and III. 0·480	I. and III. 3·44	

TABLE III.—Specific Heat. Experiments with Palladium Plate. Three different Hydrides. Weight of Plate 37·848 grms. Equivalent of Water and Calorimeter 107 grms.

	I.	II.	III.	IV.
Weight of hydrogen in pal- ladium	0·210	0·129	0·0512	Nothing.
Rise in calorimeter for a fall of 100° C.	2·891	2·694	2·368	2·111
	2·870	2·657	2·362	2·100
	2·856	2·646	2·375	2·082
	2·858	2·659	2·402	2·086
Mean rise	2·869	2·664	2·337	2·096†
Rise due to hydrogen	0·773	0·568	0·281	
Specific heat of hydrogen	3·930	4·70	5·88	

Weight of hydrogen in	Increase in calori- meter due to dif- ference of	Specific heat from difference of	Mean.
I. in excess of II. ... 0·081	I. and II. ... 0·205	I. and II. 2·70	} 3·31
II. in excess of III. 0·0778	II. and III. 0·287	II. and III. 3·94	
I. in excess of III. ... 0·1588	I. and III. ... 0·492	I. and III. 3·31	

* Mean of eight experiments.

† Mean of six experiments.

Coefficient of Expansion.

As the observations on the coefficient of expansion had to be made with the same pieces of palladium used in the former experiments, the only method that could be employed was to weigh the hydride in distilled water of different temperatures, as Matthiessen did in his well-known paper "On the Expansion of Metals and Alloys" (Phil. Trans. 1866), and to deduce the mean cubical expansion from the difference of weights and the known density of water. Experiments made in this way require great care in execution, and, when every precaution is taken, are yet liable to considerable variation. Any difference of temperature in different portions of the water in which the alloy is weighed at the time of the observation, causing currents in the fluid, or the condensation of moisture on the fine platinum wire used to suspend the substance, renders the results useless. But in this case the difficulties are greatly increased from the minute bubbles of hydrogen that are apt to accumulate at any angular point of the mass, and must be removed by suddenly depressing or lifting the mass. After making a great many observations in this way, it became clear that the method would not yield results of very great accuracy when applied to the case of palladium containing hydrogen; in the mean time the results obtained are provisionally stated. Generally a mass of palladium containing hydrogen nearly equivalent to the atomic proportion Pd^3H^2 , yields the following values for the mean coefficient of cubical expansion:—

Between 0 and 50	. .	0.000058
Between 0 and 80	. .	0.000066

The cubical expansion of palladium being 0.000033, we may say the alloy with hydrogen is just twice as great. If the expansion of Graham's alloy is assumed to be equal to the sum of the expansion of the respective volumes of the constituents, then the calculated result for the cubical expansion of hydrogen is 0.000246, a number about one and a half times the coefficient of expansion of mercury.

With a fused mass of palladium containing a small charge of hydrogen, the coefficient of expansion was found to be 0.00048, and the calculated value for the occluded hydrogen became then 0.00059.

Absorption of Hydrogen at a Red Heat.

Graham's theory of the rapid passage of hydrogen through palladium at high temperatures, assumes at first a direct absorption of the gas, and then a transmission of it by a kind of "cementation process." That an absorption of hydrogen takes

place with its attendant increase of volume at high temperatures may be shown as follows:—

Take a strip of thin sheet palladium, 4 or 5 centims. long and about 5 millims. in breadth, clamp it firmly by the end in a suitable support so that the strip is free to vibrate, and insert it edgewise in the middle of a hydrogen-flame burning from a nozzle about a millimetre in diameter. If the palladium is now depressed into the inner dark cone, it immediately begins to vibrate, producing a low musical note.

If the flame be extinguished by stopping the current of hydrogen for an instant, or allowing the gas to flow, the vibration commences again, and may be kept up without any actual flame.

The motion in this position in the flame is due to the absorption of hydrogen on the cool side next the inner cone, with its attendant increase of length, producing a bending of the sheet into the hot portion of the flame, where the hydrogen is instantly expelled from the palladium, which is forced to return to its original position from its natural elasticity.

The experiments detailed in this paper have been made with palladium very handsomely placed at my disposal by Messrs. Johnson, Matthey, and Co. of Hatton Garden, London; and I gladly avail myself of this opportunity of thanking them for the means of conducting this research.

XLII. *Determination of the Absolute Value of the Siemens Mercury Unit of Electrical Resistance.* By F. KOHLRAUSCH.

[With a Plate.]

[Continued from p. 309.]

V. *Data of Observation.*

FOR determining the three fundamental magnitudes, length, mass, and time, there were used the original platinum metre belonging to the Cabinet for Metals and Machinery in Göttingen (and kindly lent for the purpose by Professor Ulrich), a Fortin's set of platinum weights belonging to the Physical Institute, and the normal clock of the Astronomical Observatory (by which that in the Physical Observatory was regulated). Oertling's comparator, which formed the basis of all the measurements, was compared with the platinum metre. It was found that the parts of this bar at -8° were equal to the divisions of the platinum metre at 0° .

For measuring the larger distances, a wooden rod 5 metres in length, divided into centimetres and provided with a slider divided into millimetres, was used. The divisions were afterwards compared with the Oertling. This was also the case with the divisions of the paper scale which was used for observation.

Intensity of Terrestrial Magnetism.

Professor Klinkerfues had the goodness to arrange that, during the period of the observations, local magnetic influences should be avoided in the Astronomical Observatory where the variation-instruments were suspended. For observing the latter I am indebted to Dr. Riecke.

With regard to the arrangement of the bifilar magnetometer, together with accessory needle, I refer to Weber's article, "Determination of the rectangular component of the Earth's Magnetic Force in Göttingen, 1834–1853"*. The value of a division of the scale of the bifilar magnetometer at that time I found to be equal to 0·000105 in parts of the whole. The intensity corresponding to a position δ of the bifilar will be obtained in the sequel from the expression

$$T = 1\cdot83846 (1 + 0\cdot000105 \cdot \delta).$$

Absolute measurement.—The moment of inertia of the principal bar was, according to a new determination which agreed to within $\frac{1}{10000}$ with one formerly made by Weber,

$$K = 42997 \cdot 10^6 \text{ millimetre}^2 \text{ milligramme},$$

the temperature in two successive observations being 15° .

The *ratio of torsion* was 0·01085.

The *time of vibration*, ϵ , observed with the arc of p divisions, gives that reduced to zero, since the scale was at a distance of 4125 divisions from the mirror,

$$\tau \left(1 - \frac{p^2}{256 \cdot 4125^2} \right) = \tau (1 - 0\cdot00000000023 \cdot p^2).$$

If, further, during the observations of vibration δ was the mean position of the bifilar magnetometer calculated from a definite point of the scale (to which all the observations are reduced), the time of vibration actually observed must be multiplied with $1 + \frac{1}{2} \cdot 0\cdot000105 \cdot \delta$; we therefore put

$$\tau_0 = \tau (1 - 0\cdot00000000023 \cdot p^2 + 0\cdot000052 \cdot \delta).$$

Let, finally, M denote the magnetism which the vibrating bar possesses of itself—that is, in the east-west direction in which it was afterwards used as deflecting bar. In the vibrations M has a *magnetism of position*, which Weber had determined in the case of this bar to be equal to 780000 absolute units. Then

$$(M + 780000) T_0 = \frac{\pi^2 K}{\tau_0^2 \cdot 1\cdot01085} \quad \dots (A.)$$

For the purpose of making observations of deflection, the bar

* *Abh. d. Gött. Ges.* 1855, vol. vi.

M, without being displaced, was fixed in a rotating clamp, so that it could be adjusted in an east and west direction. It thereby acts upon a smaller needle (accessory needle) with a mirror, which is suspended north and south to strings which hang from a long brass rod to the top of the room. Half of the distance of these from each other, the temperature being perceptibly the same in the two determinations, was determined once for all, and was found to be

$$R = 1501.70 \text{ millimetres.}$$

The *correction-term* with $\frac{1}{R^5}$, in consequence of the ratio of the length 1 : 2 of the principal bar to the accessory needle, is brought almost to zero. By deflections from second distances it was determined that the angle of deflection ϕ at the distance R was expressed by

$$\tan \alpha = \frac{\text{const.}}{R^3} \left(1 - \frac{10300}{R^2} \right).$$

The *distance* of the mirror from the scale, the position of which was always given by the unused string, amounted to 3015 millimetres.

Ratio of torsion of the accessory needle = 0.00241.

If, finally, the mean position of the *bifilar* during the deflections be denoted by δ , we may put for the observed angle of deflection ϕ

$$\tan \phi_0 = 1 (1 + 0.000105 \cdot \delta) \tan \phi.$$

Then

$$\frac{M}{T_0} = \frac{1.00241}{1 - \frac{10300}{R^2}} R^3 \tan \phi_0.$$

Determinations of intensity were made on the 19th and 22nd of August. The *duration of the vibration* was each time observed at the beginning and at the end. There were obtained:—

	Duration, τ .	Arc, p .	Bifilar, δ .	Correction for		τ_0 .
				∞ small arc.	T_0 .	
Aug. 19. ...	sec.	diy.		sec.	sec.	sec.
	20.5709	282	+3.5	-0.0004	+0.0038	26.5743
	20.5648	205	+8.9	-0.0002	+0.0096	20.5742
					Mean =	20.5742
Aug. 22. ...	20.5793	220	-1.5	-0.0002	-0.0015	20.5776
	20.5658	246	+5.1	-0.0003	+0.0055	20.5710
					Mean =	20.5743

The observations of deflections of the accessory needle, upon which the bar M acted in the two east-westerly positions at an angle of 180° with each other, gave on a scale, the centre of which was at 770 millims. :—

		Adjustments.		ϕ .	δ .	ϕ_0 .
Aug. 19.	North...	millims. 1250·48	millims. 207·25	4 54 32	+3·5	4 54 38
	South...	1210·55	167·34	4 54 22	+6·2	4 54 35
Mean =						4 54 36
Aug. 22.	North...	1247·06	202·84	4 54 47	−0·9	4 54 46
	South...	1229·33	185·62	4 54 36	+0·5	4 54 36
Mean =						4 54 41

We find from this for the zero-point of the bifilar magnetometer the values mentioned above (page 306),

$$\begin{aligned} \text{August 19.} & \quad . \quad . \quad T_0 = 1\cdot83860 \\ \text{,, 23.} & \quad . \quad . \quad T_0 = 1\cdot83832 \\ \text{Mean} & \quad . \quad . \quad T_0 = 1\cdot83846 \end{aligned}$$

(The magnetism M of the principal bar is found in close agreement, = 538630000 or 538710000.)

From the degree of agreement of all these numbers, it seems to result that, with such means, a determination of the horizontal intensity to within an error of at most 0·1 per cent. may be safely undertaken.

Determination of Absolute Resistance.

I shall always collate the measurements of *one* kind. In so far as refers to operations which belong to any single operation out of the four, they will be discriminated by Ia., Ib., II., and III.

Terrestrial Inductor.—This has been described in Weber's paper, "Application of Magnetic Induction to the Measurement of Inclination" (*Abh. der Gött. Ges.* 1853, vol. v. p. 53). The surface of the coil is there given at 39216930 square millims. From a comparison of the standard then used with the normal metre, one part of the former is equal to 1·00086 millim.; so that the above number is changed into

$$S = 39284000 \text{ square millims.}$$

The galvanoscope (Pl. IV. fig. 1 in $\frac{1}{4}$ of the true size) consists of a multiplier of about 250 coils of copper wire 3 millims. in diameter, in 10 coils, on a wooden frame 100 millims. in breadth. The cylindrical magnets of the astatic couple are each 170 mil-

lims. in length, 14 millims. in thickness, and each of them weighs nearly 200 grms. The inner one had about 45 million units of magnetism, the outer one 2 millions fewer. (It follows from this that the local influence on the inductor, which was about 5500 millims. distant, amounted at most to the 100,000th part of the terrestrial magnetism.)

The magnets lie with two grooves in the double forks of a stirrup, which hangs to a torsion-circle with a mirror, and half encircles the multiplier. The upper horizontal part of the stirrup is extended, and bears at its ends, each at a distance of 100 millims. from the centre, two small vertical rods, which are intended to support two cylindrical brass weights perforated axially, for the determination of the moment of inertia. The weights are each 28 millims. in diameter, and have each a mass of 100 grms. After removing the upper magnet, a small rod may be fastened in the centre of the stirrup, and the weights may be placed on it on the top of each other. By a small sliding weight not seen in the figure, the centre of gravity is so regulated that the superposition of the weights does not alter its position.

The whole was suspended by a steel wire, about $\frac{1}{4}$ millim. in thickness and 2.7 millims. in length, from a beam in the roof, and was carefully protected against currents of air by a box with glass plates in the sides.

Moment of Inertia of the pair of Needles.

Mass of the weights together . . = 199970 mgrms.

External semidiameter of the same . 13.95 millims.

Internal semidiameter of the same . 0.85 millim.

Hence the moment of inertia of the two together in reference to their axis,

$$= 199970 \frac{13.95^2 + 0.85^2}{2} = 19500000 \text{ millim.}^2 \text{ milligrm.}$$

Distance of the centre of the weights from each other when they were put on the extreme projections,

$$199.824 \text{ millims. at } 17^\circ.$$

Hence common moment of inertia in the latter position, referred to the suspension wire at 17° ,

$$19500000 + 199970 \cdot 99.912^2 = 2015700000 \text{ millim.}^2 \text{ mgrm.,}$$

and at $22^\circ 5$, 2016100000 „ „

The times of oscillation corresponding to each other were always measured with nearly equal arcs, so that a reduction is unnecessary. Where there are two observations with the same load, one was always made before and the other after the second loading.

g. 2.

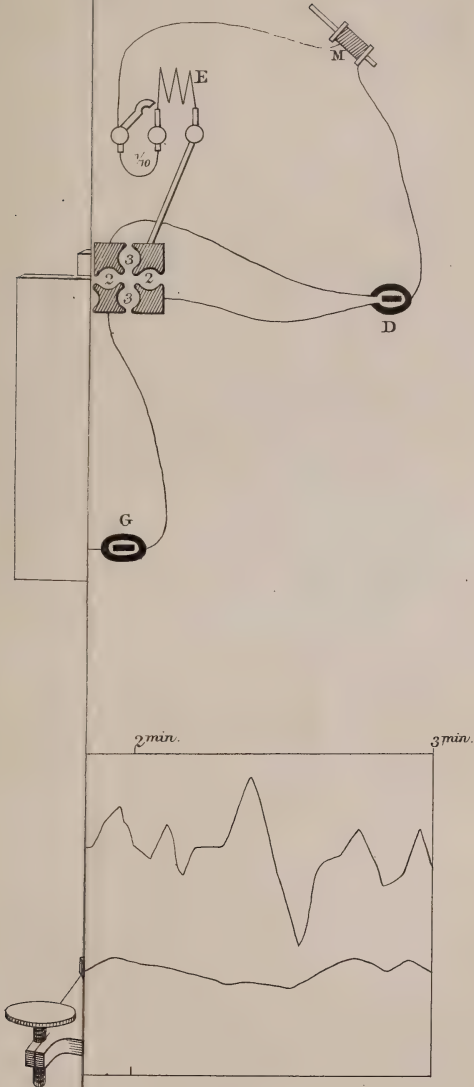


Fig. 1.

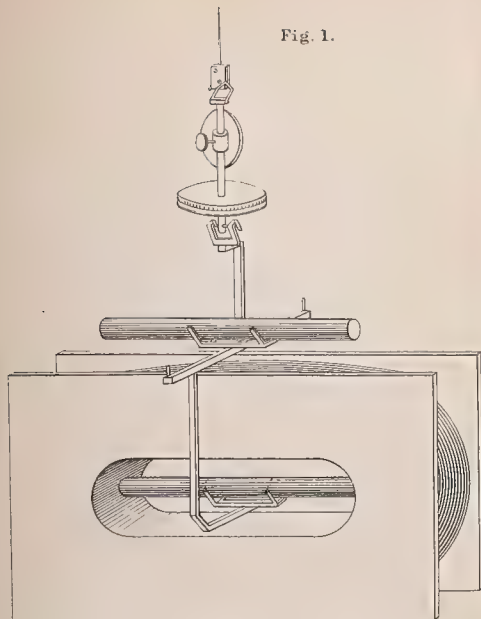


Fig. 2.

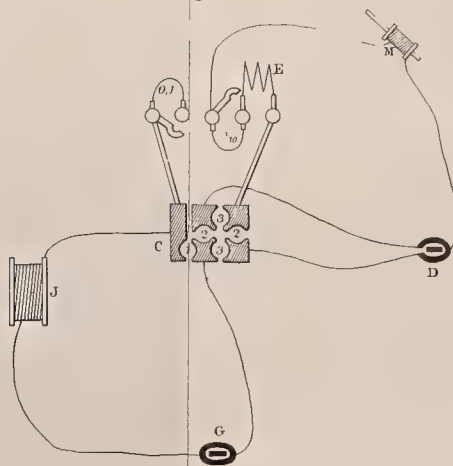


Fig. 3.

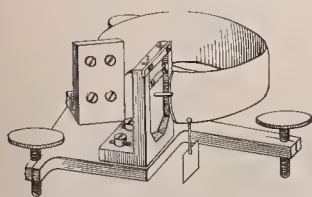
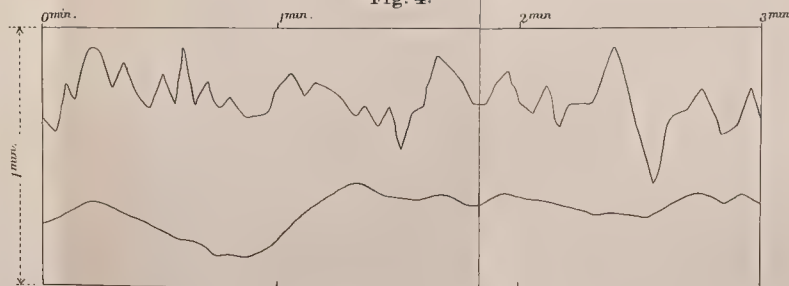


Fig. 4.



First determination.—(a) Temperature = $22^{\circ}5$. The time of oscillation of the needle-couple amounted to:—

Without load 34·0771 and 34·0692. Mean . 34·0731 sec.

With load at the ends 56·8157 „

From this we have for the moment of inertia at $22^{\circ}5$,

$$\frac{34\cdot0731^2}{56\cdot8157^2 - 34\cdot0731^2} \cdot 2016100000 = 1132400000;$$

or at 17° , ... $1132200000 = K'$.

(b) In order to eliminate the possible excentricity of the centre of gravity of the weights, each of them was turned through 180° . Temperature = 17° .

Without load 34·1486 and 34·1304. Mean . 34·1395 sec.

With load at the ends 56·9060 „

From which $K'' = 1133400000$.

Mean $K_1 = \frac{1}{2} (K' + K'') = 1132800000$ millim.² milligrm.

Second determination.—Temperature = 17° . The upper magnet was removed. It has the length 169·97, the semidiameter 6·95 millims., and the mass 199939 milligrms. Thus its moment of inertia amounts to

$$199939 \left(\frac{169\cdot97^2}{12} + \frac{6\cdot95^2}{4} \right) = 483800000.$$

The time of vibration of the other part was:—

Weights at the ends, 17·3717, 17·3720. Mean = 17·3719 sec.

Weights in the middle 8·7154 „

Hence the moment of inertia of the whole is

$$K_2 = \frac{8\cdot7154^2}{17\cdot3719^2 - 8\cdot7154^2} \cdot 1996200000 - 19500000 \\ + 483800000 = 1135700000 \text{ millim.}^2 \text{ milligrm.}$$

Now the first mode of determination is open to the objection that it has not as yet been determined whether the elasticity of torsion of a wire is entirely independent of its load. In the above case the elasticity formed the greater part of the directive force. If, therefore, on this account we assign to the second determination twofold value, the moment of inertia at 17° is

$$K = 1134700000 \text{ millim.}^2 \text{ milligrm.}$$

Of this about $\frac{7}{8}$ would be due to the magnets, $\frac{1}{8}$ to the brass parts; so that, for a temperature Θ ,

$$K = 1134700000 [1 + 0\cdot000026 \cdot (\Theta - 17)].$$

Time of vibration.—In the reduction to an infinitely small arc,

it is to be observed that only one part of the directive force depends on the terrestrial magnetism; the other is due to the elasticity of the wire. From the time of an oscillation (16 seconds of the suspension alone), the former portion is found to be almost exactly equal to half that of the second. From this there is calculated from the time t observed with the arc α , the time t_0 for infinitely small oscillations,

$$t_0 = t(1 - 0.005 \cdot \alpha^2).$$

Since t in the determination of resistance was always found to be nearly 34.4 seconds, and as the distance of the scale was equal to 4047 divisions, the correction for the arc α divisions is found to be

$$-\frac{\alpha^2}{400000000} \text{ second.}$$

To this very small correction must be added that from the clock-error. The intervals observed must be multiplied with $1 - 0.00017$, which for 34.4 seconds gives

$$-0.0059 \text{ second.}$$

In the four absolute measures there were found at the Nth oscillation the times of reversal given in the following Table, each of them calculated by Gauss's method from eight to ten observed transits through the position of equilibrium. The times of oscillation on the further side of the thick line follow from the times which are placed over against each other.

	N.	Time.		N.	Time.		Time of oscillation.			Mean. t_0 .
							Observed.	Correction.	Corrected.	
		m	s		m	s	s	s	s	s
I a. {	4	33	28.15	17	40	56.16	34.4625	-0.0061	34.4564	34.4636
	17	40	56.16	39	48	24.36	34.4769	-0.0060	34.4709	
I b. {	4	12	3.12	20	21	14.06	34.4336	-0.0061	34.4275	34.4283
	12	16	38.51	28	25	49.47	34.4352	-0.0061	34.4291	
II. {	4	11	33.47	20	20	44.40	34.4328	-0.0062	34.4266	34.4255
	12	16	8.95	28	25	19.84	34.4304	-0.0061	34.4243	
III. {	3	9	21.28	15	16	14.35	34.4222	-0.0061	34.4161	34.4140
	9	12	47.80	21	19	40.82	34.4181	-0.0061	34.4120	

Logarithmic Decrement λ_0 with interrupted Circuit.—This was taken in the well-known manner, together with the previous observations of time, by reading off the points of reversal. Care was herein taken that the arc was, on the average, about as large as in the recoil-observations. Each arc in the following Table is the mean of four adjacent ones. The numbers are reduced to infinitely small vibrations.

If a_m is the m th, a_n the n th arc, the damping-ratio is

$$K = \left(\frac{a_m}{a_n} \right)^{\frac{1}{n-m}} \text{ and } \lambda_0 = \log \text{ nat. } k.$$

	$m.$	$a_m.$	$n.$	$a_n.$	$k.$	Mean.
Ia.	3	302.81	20	247.17	1.01201	$k = 1.01194$ $\lambda_0 = 0.01187$
	7	288.48	29	222.30	1.01192	
	17	255.81	32	214.30	1.01189	
Ib.	3	313.27	20	251.14	1.01309	$k = 1.01334$ $\lambda_0 = 0.01325$
	7	298.16	24	238.15	1.01331	
	11	283.29	28	225.64	1.01347	
II.	15	268.72	32	214.02	1.01348	$k = 1.01203$ $\lambda_0 = 0.01196$
	3	388.10	20	316.94	1.01199	
	7	370.14	24	302.18	1.01200	
III.	11	352.90	28	287.80	1.01207	$k = 1.01400$ $\lambda_0 = 0.01390$
	15	336.47	32	274.39	1.01207	
	3	336.82	15	285.50	1.01387	
	7	318.93	19	269.87	1.01402	
	11	301.73	23	255.04	1.01411	

Recoil-observations.—The distance of the scale from the coated mirror amounted to 4050.6 millims., of which 5.6 millims. were represented by glass plates. The refractive index being taken equal to 1.5, we must deduct from the above number $\frac{5.6}{3} = 1.9$ millim. to compensate for the refraction of the rays of light. Thus, then, $r = 4048.7$ millims.

An amplitude of vibration a measured on the scale signifies on a circle of the radius unity the arc A^* ,

$$A = \frac{a}{8097.4} \left(t - \frac{1}{3} \frac{a^2}{8097^2} \right) = \frac{a - 0.0000000051 a^2}{8097.4}.$$

During the recoils the bifilar magnetometer was observed, and from the graphical representations of the readings the position δ (page 343) deduced for the time of each induction-shock.

I will give the set of observations Ia. in a complete form. s_1, s_2, s_3, s_4 are the points of reversal observed on the scale; the induction-shocks lie before s_1 and s_3 . Behind the thick vertical line the large arc $a = s_3 - s_1$ and the smaller arc $b = s_2 - s_4$ are given, in all cases the mean of two adjacent differences. The horizontal line indicates the distinctly marked constancy of the vibrations. This always occurred soon, as the starting-point was the initial position of the inductor, always defined by preliminary experiments, in which the first deflection had at once almost the ultimate constant value.

* If we took into account that a third of the moment of rotation is not proportional to the angle but to its sine, we should have to put $\frac{9.7}{288}$ instead of $\frac{1}{3}$. The difference is vanishingly small.

I a.

Bifilar.		Points of reversal.				Arc.	
δ .		s_1 .	s_2 .	s_3 .	s_4 .	a .	b .
		299.2	602.7	676.7	374.6	377.5	228.1
	+5.0	302.4	605.8	679.1	376.4	375.50	230.30
+4.0	+3.1	303.4	605.5	679.3	376.3	375.80	229.15
+2.4	+2.1	303.8	605.9	679.8	377.0	375.75	229.25
+2.0	+1.7	304.2	606.2	679.8	376.9	375.60	229.25
+1.5	+1.3	304.2	606.1	679.9	377.1	375.65	229.10
+1.1	+0.7	304.2	606.0	679.9	376.8	375.70	229.05
Mean $\delta = +2.3$ T = 1.8389		Mean = 375.70				229.16	
		Correction for divisions of the scale = -4.15				-2.75	
		-0.0000000051 . $a^3 = -0.26$				-0.06	
		Corrected 371.29				226.35	

Hence

$$A = \frac{371.29}{8097.4} = 0.045853,$$

$$B = \frac{226.35}{8097.4} = 0.027953,$$

$$\lambda = \log \text{nat.} \frac{A}{B} = 0.49492.$$

The other sets were obtained and reduced in the same way. I give here the arcs of vibration.

I b.			II.			III.		
δ .	a .	b .	δ .	a .	b .	δ .	a .	b .
+1.3	373.60	227.85	+12.3	374.00	228.55	+5.4	374.65	227.90
1.9	373.45	227.90	12.2	374.05	228.50	5.9	375.10	228.05
2.0	373.55	228.00	12.2	374.20	228.50	6.3	375.25	228.25
2.1	373.75	228.15	12.3	374.25	228.50	6.7	375.30	228.20
2.2	373.55	228.00	12.5	374.35	228.55	7.2	375.40	228.25
2.2	373.55	228.05	13.3	374.40	228.55	7.8	375.00	228.40
2.4	373.65	227.85				8.9	375.30	228.30
2.9	373.45	227.85				10.2	375.70	228.30
3.6	373.60	228.20				11.5	375.75	228.55
+2.3	373.57	227.98	+12.5	374.21	228.53	+7.8	375.27	228.24
	-3.97	-2.74		-4.01	-2.68		-4.00	-2.71
	-0.26	-0.06		-0.26	-0.03		-0.26	-0.06
	369.34	225.18		369.94	225.79		371.01	225.47
	A = 0.045613			A = 0.045686			A = 0.045819	
	B = 0.027810			B = 0.027884			B = 0.027846	
	$\lambda = 0.49480$			$\lambda = 0.49375$			$\lambda = 0.49802$	
	T = 1.8389			T = 1.8409			T = 1.8400	

VI. *Calculation of the Absolute Resistance.*

If we insert these numbers for A, B, λ , T, and further those for λ_0 , t_0 , K, and S in the formula (page 305)

$$w = 32 \frac{S^2 T^2}{K} \frac{t_0}{\sqrt{\pi^2 + \lambda_s^2}} \left(\frac{\lambda}{\sqrt{\pi^2 + \lambda^2}} - \frac{\lambda_0}{\sqrt{\pi^2 + \lambda_0^2}} \right) \frac{AB}{(A^2 + B^2)^2} \left(\frac{A}{B} \right)^{2 \arctan \frac{\lambda}{\pi}},$$

we obtain for the absolute resistance of the inductor and galvanometer for the four determinations, taking as unit the resistance $10^{10} \frac{\text{millim.}}{\text{second}}$, or briefly $\frac{\text{earth-quadrant}}{\text{second}}$,

$$w = \begin{array}{cccc} \text{Ia.} & \text{Ib.} & \text{II.} & \text{III.} \\ 3.9687 & 3.9937 & 3.9903 & 3.9849 \end{array} \frac{\text{earth-quadrant}}{\text{second}}.$$

The greatest difference of these numbers corresponds to a change of temperature in the wire of less than 2° , which cannot be controlled.

VII. *Comparison of the Resistance w with Siemens's Standards.*

The two standards, Nos. 1135 and 1143, each of which contained 4 mercury units, were guaranteed to be correct at 19.4° and 18.3° respectively. The increase of the resistance in the German-silver wire amounted for 1° to 0.0004 of the whole. Hence the ratio, if both were at the same temperature, must have been

$$\frac{\text{No. 1143}}{\text{No. 1135}} = 1.00044.$$

I found, by comparison with third resistances,

$$1.00050, \quad 1.00046, \quad 1.00055; \quad \text{mean} = 1.00050.$$

The difference from the above ratio corresponds to an error of temperature of only 0.15° , and may therefore serve as a test for the accuracy of the copies and of the method of comparison.

At the beginning of the observations I did not possess the standards; and instead of them I constructed four provisional German-silver resistances (I will denote them as A, B, C, D), each nearly equal to 4 Siemens's units, and which in the determination I. served for comparison with the inductor. They were subsequently compared with the standards, which had meanwhile reached me.

$$A = 4.1021 \quad B = 4.0977 \quad C = 4.1095 \quad D = 4.0965 \text{ Siemens.}$$

For interpolation two *tenths* of a Siemens unit were necessary. They were constructed of two pieces of German-silver wire, soldered in lengths of 325 millims. to copper rods. As it was found, moreover, that 3250 millims. of the same wire had the

resistance 1 Siemens at $+12^{\circ}0$, the pieces in question accurately represent tenths at that temperature.

As the circuit inductor + galvanometer had been brought to nearly 4 Siemens by the introduction of a small load of copper wire, a differential galvanometer could be used. It was externally similar to that described in page 345 *et seq.* To be quite free from thermal influences, the comparison was made by means of short currents produced by Weber's magnet inductor by the method of multiplication, and which were divided by the two resistances and the two galvanometer wires. The heating by the very feeble brief currents is in any case vanishingly small; at the same time accidental thermo-electromotive forces, which in a circuit extending over a large space are unavoidable, have no influence, as the induction-currents alternate in direction. The method used, especially with reference to the extra current produced in the inductor, has been described by me in Poggendorff's *Annalen*, vol. cxlii. p. 418; and I refer to that paper.

The arrangement of the whole of the instruments is represented schematically, fig. 2. C is a stopper commutator with six copper plates made of ebonite, massive, and very carefully worked. Near it are five solid clamping-screws, which can be placed in conducting communication with each other by a slider. The connexion of the other parts is shown by the figure. J and G, terrestrial inductor and galvanometer, form the circuit the absolute resistance of which is to be determined. These are closed at 1 by stoppers. For comparison with Siemens's standard, 1 is removed and 2 stopped. The standard E is then connected with one, and the circuit J G with the other branch of the differential multiplier D. To change them in reference to these branches, it is simply necessary to take out the stopper at 2 and insert it at 3. M is the source of the current, the magnet inductor (which was removed during the absolute measurements). The tenths added to E or J G are denoted by 0.1; by sliding and fixing the shunt they become ineffective.

The needles of the galvanometer G were, of course, fixed during the comparison. It was specially ascertained that the motion of the inducing magnet in M, which, as is well known, consists of two magnets with similar poles presented to each other, had no action at a distance on the inductor.

It is useless to detail the experiments, since the errors of comparison cannot in any case compare with those of absolute determination. Moreover the example deduced in the memoir already cited (p. 421) is one of these determinations.

Nos. Ia. and Ib. of the absolute measurements belong to a comparison made *between* them with the provisional standards, as the Siemens's standards had not then reached me. II. and

III. were made on other days, when a comparison was made both before and after. The experiments gave the resistance w , or the inductor + galvanometer, equal to the following numbers in Siemens's units:—

I. Temperature of the standard = $+15^{\circ}3$.

$$A + 0.0071 = 4.1034 \text{ Siemens.}$$

$$B + 0.0111 = 4.1030 \quad ,,$$

$$C + 0.0003 = 4.1034 \quad ,,$$

$$D + 0.0113 = 4.1020 \quad ,,$$

$$\text{Mean } w = 4.1029 \quad ,,$$

Beginning.	End.	In Siemens's units.		
		Beginning.	End.	Mean.
II. Temp. = $+14^{\circ}8$ (No. 1135) + 0.1136 (No. 1143) + 0.1118	$+15^{\circ}0$			
	+0.1108	4.1062	4.1038	4.1050
	+0.1089	4.1062	4.1036	4.1049
III. Temp. = $+13^{\circ}8$ (No. 1135) + 0.1002 (No. 1143) + 0.0980	$+14^{\circ}2$			$w = 4.1049$
	+0.1104	4.0915	4.1022	4.0968
	+0.1086	4.0906	4.1019	4.0962
				$w = 4.0965$

VIII. Results.

Comparing these resistances expressed in Siemens's units with those expressed in absolute measure, we obtain

$$\text{I. } 4.1029 \text{ Siemens} = 3.9812 \frac{\text{earth-quadrant}}{\text{second}}; 1 \text{ Siem.} = 0.9703$$

$$\text{II. } 4.1049 \quad , \quad = 3.9903 \quad , \quad ; \quad , \quad = 0.9721$$

$$\text{III. } 4.0965 \quad , \quad = 3.9849 \quad , \quad ; \quad , \quad = 0.9728$$

In the mean, therefore,

$$1 \text{ Siemens's mercury unit} = 0.9717 \frac{\text{earth-quadrant}}{\text{second}}^*.$$

With regard to the ratio of the British-Association unit to Siemens's unit, the most trustworthy value hitherto published is probably that which M. Dehms deduced from a comparison by Mr. Fleeming Jenkin†,

$$1 \text{ British-Association unit} = 1.0493 \text{ Siemens.}$$

M. Dehms and Mr. Hermann Siemens had the goodness to make at my request a new comparison, from which a British-Association unit (No. 61) belonging to Siemens's laboratory was

* In the result 0.9705 (*Gött. Nach.* 1870, p. 523) an error of calculation had crept in.

† *Pogg. Ann.* vol. cxxvi. p. 404. Report of the British Association, 1864, p. 349. *Phil. Mag.* vol. xxix. p. 477.

Phil. Mag. S. 4. Vol. 47. No. 313. May 1874. 2 A

found to be $=1.0473$. Owing to an injury to the unit, the comparison had to be made in the air; and therefore no great weight must be assigned to it. The British-Association units belonging to M. Brix (No. 21) and M. Weber (No. 51) were also compared, and gave, in accordance with the above, the value 1.0493 . If this agreement be compared with the former enormous differences in the statements regarding units of resistance, a very satisfactory proof is afforded as to the progress in this kind of measurement*.

Taking the number 1.0493 , it will be found that this unit is nearly 2 per cent. more than was intended.

The *electromotive forces* of Daniell and Grove I have found, in conjunction with Ammann, to be $=11.71$ and 19.98 Siemens-Weber units respectively; they have therefore the absolute value†

$$\text{Daniell} = 11.38 \times 10^{10} \frac{\text{millim.}^{\frac{3}{2}} \text{ milligrm.}^{\frac{1}{2}}}{\text{second}^2}.$$

$$\text{Grove} = 19.42 \times 10^{10} \quad ,,$$

The thermoelectric force of German-silver-iron, expressed in the same units, $=2400000$ for a difference in the junctions of one degree at mean temperatures.

XLIII. Direct Solution of a Geometrical Problem.

To the Editors of the Philosophical Magazine and Journal.

GENTLEMEN,

Gonville and Caius College,
Cambridge, March 27, 1874.

MAY I request the publication in your Magazine of the accompanying paper and letter which came from a lady in California? The paper, as you will see, is a solution of a geometrical problem which, more than twenty years ago, excited considerable interest by its discussion in your Magazine. The Vice-Chancellor of Cambridge, to whom these documents were addressed, placed them in my hands; and at his request I forwarded them to Professor Sylvester, who expresses the opinion, with which I entirely agree, that the solution is thoroughly sound, and authorizes me to say that he has suggested the propriety of the publication of the papers in your Magazine. In this suggestion the Vice-Chancellor cordially concurs.

I am, Gentlemen,

Your obedient Servant,

N. M. FERRERS.

* The research will be found in Poggendorff's *Annalen*, vol. cxlvii. p. 155.

† Compare Pogg. *Ann.* vol. cxli. p. 458, wherewith it must be remarked that, after the necessary reductions, the numbers which Ammann and I have found agree very closely with the results of Waltenhofen (Pogg. *Ann.* vol. cxxxiii. p. 478).

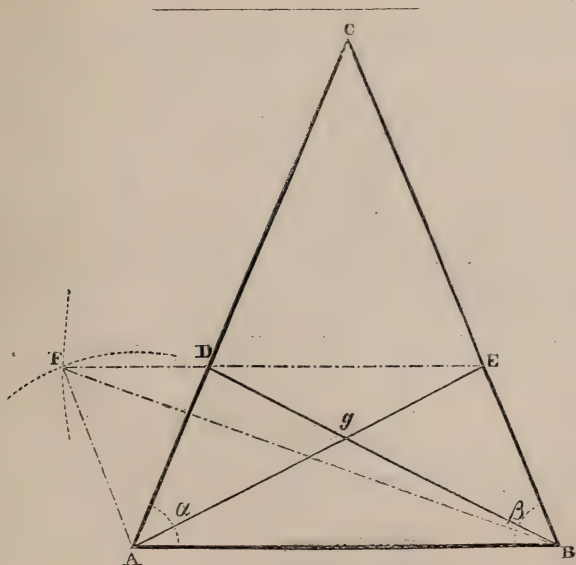
Oakland, Feb. 15, 1874.

SIR,—In a Number of the London, Edinburgh, and Dublin Philosophical Magazine for the year 1852 or 1862, is a discussion of a simple geometrical problem by J. J. Sylvester, in which this gentleman attempts to illustrate a conjectured principle in the Theory of Geometrical Method. The problem appears to have been given out at Cambridge many years before, and to have excited the attention of some of the first mathematicians of Europe for a number of years. A direct solution of the problem was demanded ; and after a protracted discussion it was concluded that none was possible. Upon that some mathematicians lent their attention, by a study of the nature of the problem under question, to discovering some rule by which on inspection they might be able to tell whether a given problem admitted of only an indirect solution, a question of some importance in astronomy.

I have not been able to discover that any direct solution has been brought forth as yet; so I send you this, given to me by a friend of mine, Mr. Hesse, who solved it in 1842. I send it, thinking that every mistake cleared up in science is a step toward truth.

If it would not be too much trespassing on your good nature, I should be pleased to hear of the fate of my solution.

CHRISTINE CHART.



In the triangle ABC , two angles, CAB and CBA , are

bisected, and the portions of the bisecting lines $A E$ and $B D$ contained between the vertices A, B and the opposite sides $B C$ and $A C$ are equal: to prove that the angles α and β are also equal.

Find a point F , so that $F A = A D$ and $F E = A B$, and draw line $F B$.

I. We have

$$\left. \begin{array}{l} A F = A D \\ F E = A B \\ A E = D B \end{array} \right\} : \text{follows the equality of triangles } F A E \text{ and } A D B.$$

II.

$$A D B + \frac{\alpha}{2} = D g E,$$

$$B E A + \frac{\beta}{2} = D g E.$$

Therefore

$$A D B + \frac{\alpha}{2} = B E A + \frac{\beta}{2}.$$

But

$$\left. \begin{array}{l} A D B = F A E, \\ F E A = \frac{\beta}{2} \end{array} \right\} \dots \dots \dots (I)$$

Therefore

$$F A E + \frac{\alpha}{2} = B E A + F E A,$$

or

$$F A B = B E F.$$

Also

$$F B = F B,$$

and

$$F E = A B.$$

III. From which follows the equality of triangles $F B A$ and $F B E$, considering that

$$\text{angles } F A B = F E B = B E A + \frac{\beta}{2} = D g E. \dots (II.)$$

But

$$D g E = 180^\circ - \frac{\alpha + \beta}{2},$$

$$\alpha + \beta < 180^\circ,$$

and

$$\frac{\alpha + \beta}{2} < 90^\circ;$$

therefore

$$D g E \text{ or } F A B > 90^\circ.$$

From III. follows $FA = BE$; } therefore $BE = AD$,
 and by construction $FA = AD$; }

from which results the equality of triangles ABE and ABD ,
 and therefore $\alpha = \beta$. Quod erat demonstrandum.

F. G. HESSE.

XLIV. On a new form of Calendar, by which the Year, or Month, or Month-day, or Week-day may be readily found when the other three components of a date are given. By Captain J. HERSCHEL, R.E.*

THE Tabular Calendar annexed is perhaps as compact in form and simple in use as is possible. Its use may be stated thus:—Of the Year, Month, Day, and Week-day, given any three to find the fourth. If we arrange these thus,

	M	
D		W
	Y	

, it is clear that M and D, Y and W will each fix one

of the signs or hieroglyphs in the centre of the Table; and in order that the day of the month shall fall on a certain week-day in any given year, these signs must be the same. Thus,

	Apr.	
13		Mon.
	1874	

take the case of April 13, and Monday in

1874, both indicate *. We have now only to suppose one of these unknown to see that the sign indicated by the other pair will show all the possible solutions. Thus;—

1. Y unknown; M and D indicate * (or any other sign, according to the case); W and * indicate Y.
2. M unknown; Y and W indicate *; D and * indicate M.
3. D unknown; Y and W indicate *; M and * indicate D.
4. W unknown; M and D indicate *; Y and * indicate W.

It is obvious that the signs are pure symbols for the occasion, and are for this very reason chosen heterogeneous so as not to have any *meaning or order, per se*.

It is only necessary to add that, where the date is known to fall in January or February of a bissextile year, the *italic* M is to be used. Otherwise, or more generally, the *italic* M is *only* and *always* to be used with a bissextile Y. Thus:—

* Communicated by the Author.

XLV. *On the Physical Cause of Ocean-currents.*

By WILLIAM B. CARPENTER, M.D., LL.D., F.R.S.

To the Editors of the Philosophical Magazine and Journal.

University of London, Burlington House, W.

April 22, 1874.

GENTLEMEN,

MR. J. CROLL, in a note to his last paper "On the Physical Cause of Ocean-currents" (*Phil. Mag.* for March, p. 176), charges me with the following inaccuracy:—

"Dr. Carpenter states that 48 miles per day is the mean annual rate of the Gulf-stream in the 'Narrows;' but in the Admiralty's Current-chart, published October 1872, the minimum rate is stated to be 32, and the maximum rate 120 nautical miles per day. This gives 87 statute miles per day, or fully $3\frac{1}{2}$ miles per hour, as the mean rate."

Not being a Statistician, as Mr. Croll professes to be, I should have never thought of deducing a mean rate from a *maximum* and *minimum* only, having always supposed that a correct *average* could only be based upon a large number of data, the larger the better. Thus, if I wanted to know the *average* number of children of one marriage, I should not take the *arithmetical mean* between a *maximum* of (say) 20, and a *minimum* of 0, which would give 10; but should collect 1000, 10,000, or 100,000 observations, and find the mean of the whole.

This last is precisely what the Meteorological Department, on whose guidance I have relied in this matter, have done in regard to the rate of the Gulf-stream. If Mr. Croll will look into their "Currents and Surface-Temperature of the North Atlantic" (1872), he will find the following passage (p. 23) under the head of "Remarks on the General Current Chart, constructed from *the whole of the observations* on the currents employed in this discussion, irrespective of the time of the year in which they were made."

"The main part of the stream is forced into the Straits of Florida, turning to the N.E. by E., in its passage between the Florida Reefs and Cuba, at a mean rate of 39 miles. About lat. 25° N. the straits between Florida and the Bahamas Bank become contracted, part being known as the Narrows. Here the set becomes almost due N., and its rate greater, averaging 48 miles. This rate and set is maintained to lat. 30° N."

If, again, Mr. Croll will look into the General Chart itself, he will find the first of these averages to be based on thirty-eight observations, the second on fifty-four observations, and the third on forty observations.

Or, if he will examine the Monthly Charts, he will find that while *six* of them give a monthly average *below* 48 miles per day, there is only *one* that gives an average so high as 73.6 miles a day. The average of the six months of *least rapid* rate is 34

miles per day ; the average of the six months of *most rapid* rate is 60·6 miles per day.

Further, as the *maximum* rate given by the Meteorological Department is 106 miles per day, and as this rests on only a single observation, it is obvious that the Admiralty maximum of 120 miles per day is a case as exceptional as that of a man who has twenty children by one wife.

The annual mean assumed by Mr. Croll, on the basis of the maximum and minimum rates, thus exceeds the very highest actual monthly average, and is more than double the monthly average of half the year ; whilst the annual mean stated by me, on the express authority of the Meteorological Department, is the average of *all* the observations in its possession.

The foregoing case furnishes a typical example of the arithmetical method on which Mr. Croll relies, in his laboured disproof of the doctrine of a Vertical Oceanic Circulation sustained by Thermal Agency alone, which has received the full sanction, on theoretical grounds, of numerous eminent Physicists (as, for example, of Sir George Airy, in his Presidential Address to the Royal Society in 1872), and which appears to the French Academicians before whom I recently brought it (*Comptes Rendus*, April 6), to be in entire harmony with the facts of Ocean-temperature as determined by the 'Challenger' observations.

But, further, Mr. Croll asserts (p. 99) that "all we really require to account for the cold water which is found to occupy the bed of the ocean in temperate and equatorial regions," is a set of polar under-currents maintained by the winds which impel equatorial water towards the poles ; and further (p. 175), that, "so far as the distribution of heat over the globe is concerned, *it is a matter of indifference whether there really is or is not such a thing as this general oceanic circulation.* The enormous amount of heat conveyed by the Gulf-stream alone puts it beyond all doubt that ocean-currents are the great agents employed in distributing over the globe the excess of heat received by the sea in intertropical regions."

Regarding the *facts* of the case as better data than Mr. Croll's *figures*, I would submit whether the following considerations do not furnish a *reductio ad absurdum* of the foregoing statements:—

1. In the Equatorial Atlantic, as is shown by the 'Challenger' sections, the *whole mass* of water, from 300 fathoms to the bottom at (say) 2500 fathoms, has a temperature descending from 40° to 32°. The *isocheimenal* of the Equatorial Atlantic (which, as is shown by the temperature-phenomena of the Mediterranean and Red Sea, would be the temperature of all but the superficial stratum throughout the year, if Polar water were excluded) is about 75°. Hence it is clear that the great bulk of this water must have travelled to the Equator from the Polar areas. That

(1) this enormous mass of glacial water, of which, across the whole breadth of the South Atlantic, the lowest stratum of 500 fathoms has an average temperature below 35° , exerts no cooling influence on the Intertropical area, and that (2) it can have been propelled thither by the agency of the Gulf-stream or of any other superficial wind-currents, seem to me propositions equally opposed to common sense.

2. That Polar water is met with at a much less depth under the Equator than it is in the extratropical zone, seems a clear indication that it is *continually rising* there from the bottom towards the surface, as would of course be the case on the theory of a Vertical Oceanic Circulation; and this inference is strikingly confirmed by the fact (which is diametrically opposed to another of Mr. Croll's assertions, p. 100), that while the salinity of the *bottom-water* maintains its low Polar standard as far as the Equator, and while the salinity of extratropical *surface-water* is considerably higher, *the salinity of Equatorial surface-water is as low as that of the Polar bottom-water.*

3. Of the Polar-Equatorial flow thus evidenced, a poleward flow of the *whole upper stratum* is a necessary complement; and the observations made by my colleagues and myself between Lisbon and the Faroe Islands, indicate that the thickness of this north-moving stratum is there not less than 700 fathoms; whilst the course of Dr. Petermann's isothermal lines clearly shows that it extends *all across the Atlantic* to Newfoundland, where the summer isotherms turn almost due north. That this vast body of water can be propelled by the *vis a tergo* of the true Gulf-stream (which off Sandy Hook has a breadth of about 60 miles and a depth of 100 fathoms), I have not heard of any one but Professor Wyville Thomson, Mr. Croll, and Dr. Petermann, who would seriously assert. That it exerts no heating influence, not even Mr. Croll would affirm.

4. That the *real* Gulf-stream, or Florida Current, reaches North-western Europe, is a pure assumption of geographers and meteorologists, to account for the abnormally high temperature brought thither by the sea. On evidence which no one has disproved, it may be affirmed that the real Gulf-stream dies out, *i. e.* loses all the distinctive characters of a current, in the Mid-Atlantic. On the other hand, a *vera causa* satisfactory to Physicists has been shown to exist for the poleward movement of the entire upper stratum. And thus Professor Möhn, of Christiania, in sending me his work 'On the Climatology of Norway' (1872), which assigned to the Gulf-stream the mild winter of the Norway coast, informed me that if he had been acquainted with my latest Report he should have expressed himself very differently, being now satisfied that this amelioration is produced, not by the Florida Current, but by the General

Oceanic Circulation,—as is also Dr. Meyer, who has been engaged for some years in the study of the Physics of the Baltic and the North Sea.

If Mr. Croll cannot apprehend a Physical conception which such men as Sir William Thomson and M. Dumas (who have kindly allowed me thus to cite their authority) accept as having a valid scientific basis, I submit that the fault is not mine, but his; and shall not feel called upon to prolong a discussion which will obviously never convince *him*. My time will be much better employed in working out an experimental investigation by which I have reason to hope that further light will be thrown upon the matter.

I remain, Gentlemen,

Your obedient Servant,

WILLIAM B. CARPENTER.

XLVI. *A Variation-Barometer.* By F. KOHLRAUSCH*.

[With a Plate.]

A BAROMETER which combines with unbounded sensitiveness the absence of any friction, which requires only a single observation to be made that consumes no time, and possesses so slight a moment of inertia that it follows the variations of the atmospheric pressure in the fraction of a second, will probably excite some interest; wherefore I will describe the simple way in which I have produced such a one (Plate IV. fig. 3).

The air-exhausted metal ring from a Bourdon aneroid† is on one side firmly screwed on a holder; the other, free end pushes with its rounded projection against a small mirror in a metal frame, which is suspended on little steel springs (strips of pendulum-steel). An axis, which I first tried, even the finest, rendered the sensitiveness illusory; I had therefore put aside as useless the apparatus already four years old. The mirror-holder can be shifted a little on the bottom-plate by means of two notches; at the lowest height of the barometer the mirror must rest more securely against the projection. A small wing, soldered to the ring, and dipping deep into the liquid (glycerine) contained in a vessel, serves to rapidly still any vibrations.

The instrument is set up firmly upon a wall-stand, best by means of a screw through the centre of the bottom-plate; after regulating it with the adjusting-screws; and then the observations are effected in the usual manner with telescope and vertical scale‡.

My scale is placed at about 3 metres distance from the mirror.

* Translated from a separate impression, communicated by the Author, from Poggendorff's *Annalen*, vol. cl. p. 423.

† I have obtained such tubes through M. Apel, University Mechanician, Göttingen.

‡ M. Waibler, Mechanician, Darmstadt, supplies the apparatus for 12 thalers.

Thus nearly 25 scale-divisions correspond to 1 millim. of the mercury barometer; so that the variations of pressure which occur with us require a scale a metre in length. Sudden opening or closing the door of the observing-room causes the image to move over 2 or 3 divisions. A further augmentation of the sensitiveness could be obtained without difficulty by increasing the distance of the scale or placing the point of contact higher, but might not answer any purpose.

The instrument is, of course, intended for observations within a brief space of time; nevertheless, in the course of an entire month, mine changed its position relative to a mercury barometer by only about $\frac{1}{4}$ millim. of the latter.

The influence of temperature was uncommonly great, viz. 0.3 millim. for 1° . As a fall of the position corresponds to a rise of temperature, it is evident that the phenomenon arises from too much air being contained in the tube, and that it can be easily obviated. Since we have, as two opposite corrections, the diminution of the elasticity of the metal and the increase of that of the internal air, it must be possible even to reduce to zero the temperature-correction of any aneroid barometer by fixing its content of air at a determinate quantity. From the series of observations which I have made occasionally in the course of the last few months it follows that, as was to be expected, the pressure of the air seldom remains constant, even for a short time. For the most part, very small variations are constantly taking place. It is different, however, at times of high wind. The upper curve, fig. 4, gives for instance the course during three minutes on the 26th of February last, a stormy day. The entire height of the figure corresponds to 1 millim. of mercury; so that the greatest fluctuation denotes $\frac{1}{2}$ millim. In the observing-room a window, turned away from the direction of the wind, was opened. The noting-down took place every three seconds; nevertheless a great number of small variations could not be recorded. The lower curve exhibits the progress during a moderate hail-shower accompanied by a thunder-storm on May 30, likewise in the course of three minutes. The tempest had commenced some minutes previously. The smaller undulations which follow the initial larger wave continue, diminishing, for some time.

The past summer afforded frequent opportunities of observation during violent storms; yet I have never been able to detect any connexion between the oscillations and lightning.

I must remark in conclusion, that M. Röntgen has employed the mirror-reading in another way for measuring the pressure, and also projected an aneroid barometer with mirror and scale*.

* Pogg. *Ann.* vol. cxlviii. p. 624.

XLVII. *Notices respecting New Books.*

An Elementary Treatise on Quaternions. By P. G. TAIT, M.A.,
Formerly Fellow of St. Peter's College, Cambridge, Professor of
Natural Philosophy in the University of Edinburgh. Clarendon
Press Series. Oxford. 1873. (Pp. 296.)

THIS is the second and enlarged edition of a work first published in the year 1867. It is designed to render the subject of Quaternions "intelligible to any ordinary student;" but it must be understood that the "ordinary student" means one who is already familiar with such subjects as Surfaces of the Second Order, Homogeneous Strain, the Theory of Double Refraction, Electrodynamics, &c. as commonly treated, and so is likely to be interested in seeing the same subjects treated by a new method. It must be a matter of congratulation to all who are interested in Quaternions that a second edition of the present work should be called for after the lapse of only six years; and this fact probably justifies the opinion of the author, that "there seems now at last to be a reasonable hope that Hamilton's grand invention will soon find its way into the working world of science."

Passing to the contents of the volume, we may say that the first five chapters are devoted to an exposition of the principles of the science, viz. to Vectors and their composition, to Products and Quotients of Vectors, to interpretations and transformations of Quaternion expressions, to differentiation of Quaternions, and to the solution of Equations of the first degree. These chapters occupy 103 pp. The remainder of the book is taken up with applications—in the first place to Geometry, in the next to Kinematics and Physics.

Owing to the great variety of subjects treated, it is not easy to give a satisfactory idea of the contents; perhaps our best course will be to state with some minuteness the contents of a single discussion, and leave the reader to draw his own inferences. For this purpose we will take the articles on Homogeneous Strain, a branch of Kinematics which Professor Tait has made his own, having published three distinct accounts of the subject*. The author first shows that the determination of a vector whose direction is unchanged by strain depends on the solution of a cubic equation with real coefficients, and obtains the form which this equation takes when the mass is rigid. He then shows that a mass initially spherical becomes an ellipsoid after strain, and, on the other hand, that a mass spherical after strain was ellipsoidal before strain, the axis of the ellipsoid in either case corresponding to a rectangular set of three diameters of the sphere. After defining a pure strain, he obtains a criterion by which to distinguish it from other strains, and proves that two pure strains successively applied give a strain accompanied by rotation. "The

* Viz. 'Treatise on Natural Philosophy,' vol. i. pp. 99 &c., 'Elements of Quaternions,' p. 210 &c. (the work noticed in the present article), 'Introduction to Quaternions,' p. 180 &c.

simplicity of this view of the question leads us to suppose that we may easily separate the pure strain from the rotation in any case, and exhibit the corresponding functions.* In fact, when the linear and vector function expressing the strain is not self-conjugate, it may be broken up into pure and rotational parts in various ways, two of which are specially noticed:—the *first* leading to the theorem (which does not seem to have been previously noticed) that there is always one and only one mode of resolving a strain into the geometrical composition of the separate effects of (1) a *pure* strain, and (2) a rotation accompanied by uniform dilatation perpendicular to its axis, the dilatation being measured by $(\sec \theta - 1)$, where θ is the angle of rotation; the *second* leading to the result that a strain ϕ is equivalent to a pure strain $\sqrt{\phi' \phi}$ followed by a rotation $\frac{\phi}{\sqrt{\phi' \phi}}^*$. An expression for the angle be-

tween any two lines and planes in the altered state of the body is then found, and the locus of equally elongated lines shown to be a cone of the second order. Finally, the properties of a simple shear are demonstrated. This is perhaps a fair specimen of the contents of the volume. It contains a large number of theorems, most of them well known, worked out by a new method, while here and there a new result is obtained.

In fact one of the main objects of the work is to show how questions hitherto worked out in other ways can be treated by Quaternions; and particularly to do this with respect to questions of physics. It seems to have been in consequence of applications to such questions that Sir W. R. Hamilton wrote the words:—"Professor Tait, who has already published tracts on other applications of Quaternions, mathematical and physical, including some on Electrodynamics, appears to the writer eminently fitted to carry on happily and usefully this new branch of mathematical science, and likely to become in it, if the expression may be allowed, one of the chief successors to its inventor." (Elements, p. 755, note.)

To work out old results by the new method is an indispensable preliminary to a further advance; and that further advances will, in fact, be made by the new method seems to be the general opinion of those who have mastered it. Perhaps the case may not be unlike that which actually occurred in the progress of Physical Astronomy. Newton's successors found it necessary to work out his results by new methods before they could advance further; and to repeat his work in a new form required no small expenditure of labour and genius. To what extent the resemblance will hold good in the present case remains to be seen; but this at least is certain, that the Quaternion methods enable us to solve with ease many questions which are very difficult when attempted in other ways.

* The symbol ϕ denotes the operation by which any unstrained vector ρ becomes the strained vector $\phi\rho$; ϕ' is an operation conjugate to ϕ .

A second object which the author had in view was to produce "a really elementary treatise" on the subject. And this he has in one sense accomplished; for the exposition of the subject is not unduly long or hard, except so far as the subject itself is hard; and a student who is already familiar with the matter to which the method is applied in the last two hundred pages of the volume, would probably master its contents without very serious difficulty. Still we think something more might be done in the way of simplification. The student is in danger of being bewildered at the outset by a number of symbols which look like what he has already had to do with, but which must be interpreted in wholly new and unexpected ways. This difficulty yields to labour; but it might be sensibly diminished by being brought more gradually before the learner. There is the less need to dwell on this, as a still more elementary treatise on Quaternions* has been recently published (written in part by Professor Tait), in which the treatment is perhaps as simple as the subject admits, and which could be made out by any one to whom the study of the subject would be advantageous.

The Expression of a Quadratic Surd as a continued fraction. By THOMAS MUIR, M.A., Assistant to the Professor of Mathematics in the University of Glasgow. Glasgow: J. Maclehose. 1874. (8vo, pp. 32.)

If $Gx^2=H$, where G and H are integers, it is well known that x can be expressed as a recurring continued fraction in the form

$$A + \frac{1}{a_1 + \frac{1}{a_2 + \frac{1}{a_3 + \dots}}},$$

and that the first term of the first recurring period or cycle is a_1 , assuming H to be greater than G . In the calculation of the numbers $A, a_1, a_2, a_3, \dots$ two other sets of numbers are used—viz. a set of divisors $G, d_1, d_2, d_3, \dots$ and a set of rational dividends $0, AG, r_2, r_3, \dots$. The pamphlet before us is a systematic investigation of the properties of these sets of numbers and of the successive convergents of the continued fraction. It differs from the accounts of the subject to be found in ordinary text-books by the greater generality and completeness of the treatment of the subject. *E. g.* it is usual to notice no more than the case in which $x^2=H$; but according to Mr. Muir's treatment this is only a particular case having some special properties, which he discusses in the last six pages of the pamphlet.

The cycle formed by the successive quotients is well known to take the form $a_1, a_2, \dots, a_2, a_1, 2A$. Now there is, of course, a means of deriving each convergent successively from that which precedes it; but the form of the above cycle suggests that there ought to be some simple method of determining certain convergents without calculating all the intermediate ones; and this is in fact the

* Introduction to Quaternions, by Professors Kelland and Tait.

case. Thus it is found that

$$\sqrt{\frac{18}{7}} = 1 + \frac{1}{1+} \frac{1}{1+} \frac{1}{1+} \frac{1}{1+} \frac{1}{10+} \frac{1}{1+} \frac{1}{1+} \frac{1}{1+} \frac{1}{1+} \frac{1}{2+...}$$

From this the following convergents can be derived in succession:—the first six, $\frac{1}{1}, \frac{2}{1}, \frac{3}{2}, \frac{5}{3}, \frac{8}{5}, \frac{85}{53}$; the tenth, $\frac{449}{280}$; the twentieth, $\frac{403201}{251440}$, and so on to the fortieth, eightieth, &c. Mr. Muir investi-

gates in detail the properties of the successive convergents by which these and similar results are obtained. The notation used in the pamphlet is unusual; and though it is easy to imagine the reason for its adoption, that does not prevent its being cumbrous; *e. g.* if the fraction written at the beginning of this article is under consideration, the equation, which is commonly written thus,

$$p_n q_{n-1} - p_{n-1} q_n = (-1)^{n+1},$$

assumes in Mr. Muir's notation the following form:—

$$K(A, a_1 \dots a_n) K(a_1 \dots a_{n-1}) - K(A, a_1 \dots a_{n-1}) K(a_1 \dots a_n) = (-1)^{n+1}.$$

The pamphlet is designed for the use of students, who will doubtless find it a useful supplement to their text-books.

The Universe and the Coming Transits. By RICHARD A. PROCTOR, B.A. Cantab. London: Longmans, Green, and Co. 1874.

We shall not easily forget the intense delight with which we perused many of the essays, on their first appearance, that Mr. Proctor has collected in the present volume; and it is with some regret that on reperusing them in their collected form we have not experienced the same pleasure as on the occasions alluded to. Perhaps it is inseparable from the mode of publication adopted that a considerable amount of repetition occurs, so much so as to be painful in reading essay after essay on the transcendental subject of the "construction of the universe;" and we cannot help thinking that Mr. Proctor would have much better served this the highest branch of astronomy had he given us the cream of each essay in consecutive order, commencing with a brief history of its development instead of including this essential part in one of the later essays of his series.

The greatest problem connected with an exposition of the universe as illustrative of the construction of the heavens is doubtless that of the constitution of the Milky Way. We have experienced some little difficulty in ascertaining clearly Mr. Proctor's views of the *Via Lactea*, principally on account of the fragmentary character of the work before us; but we have marked a few passages in which we apprehend Mr. Proctor's views are definitely stated. On p. 83 we read:—"All these phenomena point to the conclusion that the Milky Way, in this neighbourhood at any rate, is really what it appears to be,—a belt or zone of stars separated from us by a com-

paratively starless interval." On p. 104 we meet with this statement:—"If the Milky Way be really as it seems . . . a stream of stars of many different orders with an enormous preponderance of relatively small stars;" and on p. 194 Mr. Proctor, referring to his correspondence with the late Sir John Herschel, remarks, "It will be seen that Sir John Herschel here maintains at once the reality of the aggregation of the brighter stars on the *visible* Milky Way, and the justice of the statistics which related to the Milky Way regarded as a *zone*. This is undoubtedly the true explanation of the matter, as my equal-surface chartings have since abundantly demonstrated." From these extracts we gather that Mr. Proctor regards the Milky Way as a zone or belt of stars which includes many different orders, the smaller stars preponderating; and these he endeavours to show in other parts of his essays to be greatly inferior in size and brilliancy to the lucid stars, which he says, on p. 194, "do unquestionably crowd upon the real Milky Way—that altogether irregular region of star-streams and star-clusterings which has been aptly compared to a band of broken *cirrus* clouds."

In several of the matters brought before the reader in these Essays Mr. Proctor claims "originality of conception;" and it is of some importance that these claims should be substantiated. Any one looking at the beautiful charts (plate 8) showing the distribution of star-clusters and nebulae with the Milky Way encompassing the heavens, must come to the conclusion that the Galaxy is an *apparently* reentering lucid zone; but is this idea really *new*? Is it not rather the development of conceptions originating in the mind of Wright, of Durham, who, supposing the sun to be plunged in a vast stratum of stars of inconsiderable thickness compared with its dimensions in other respects, considered that it was not difficult to see that the appearance of the heavens might be explained? Mr. Proctor has been at vast pains to destroy the preexisting theories of the Herschels and Struve; but are they not successive steps in the process of unravelling the complex and tangled phenomena which alone are cognizable to us from our stand-point on the surface of our little globe, circling around a star by no means, as we suppose, the largest in the universe? Might not Mr. Proctor have saved himself the labour of writing down these theories? for assuredly no man has as yet given to the world at first a perfect theory of any one group of phenomena in the natural world. The earlier conceptions of Wright in 1734 were moulded into shape by the elder Herschel, and again those of his father by the younger Herschel; for, as Mr. Proctor states on p. 84, "In the conclusions just deduced I am in agreement with Sir John Herschel, who indeed says in one place that the Galaxy, looked at according to a certain view, would 'come to be considered as a flat ring.'" The idea of the zone or belt was entertained previously to Mr. Proctor taking up his pen, the difference being (and an important difference it is) that whereas Sir John Herschel considered the ring to be flat, Mr. Proctor, if we mistake not, regards it as having the form of a hoop, its breadth being generally at right angles to the visual

ray. He does not indeed say this; but we gather the idea from many passages in his Essays.

Next in importance to the Milky Way is the zone of lucid stars which fringes, and indeed intersects the Galaxy as seen from our stand-point on earth. It is in the treatment of this zone of lucid stars that we trace the commencement of Mr. Proctor's development of his views of the starry heavens. This zone he conceives as consisting of a stream of stars, the components of which are immersed in, and intimately associated with, the Milky Way. In connexion with this association, Mr. Proctor lays considerable stress on the irregularity of outline of the Galaxy produced by the long lateral offsets consisting of wisps and sprays of the really small stars composing that wonderful zone, and adduces instances of lucid stars occupying the apices of sharply defined projections from the Milky Way.

Closely associated with the galactic zone are found those remarkable objects in the heavens, the irregular nebulae, which, with very few exceptions, as stated by Mr. Proctor, are found on the Milky Way. "Corresponding," says our author on p. 86, "to the association between the Milky Way and lucid stars is a phenomenon which falls here to be considered . . . the irregular nebulae . . . the wisps and sprays of nebulous light which stream from the central convolutions of such nebulae correspond, quite closely in many instances, with streams of small fixed stars. In many of these nebulae also there are streams of faint nebulosity extending towards fixed stars, and acquiring a sudden brightness around them. In connexion with the views of Mr. Proctor, based on these apparent associations, we would remark that our author, in our opinion, depends almost solely on the application of the theory of probabilities for substantiating his views. Now probability is not proof; and before a theory can or ought to be received as an exponent of natural phenomena, it is necessary that it be demonstrated; and until this is effected, the theory remains in the category of guesses. A true guess it may be nevertheless; but it requires *crucial observation* to establish it. In the quotation from p. 83 Mr. Proctor speaks of the Galaxy as being separated from us by a comparatively starless interval; but in no part of the work does he indicate, as we can find, where this starless interval is situated. We know the sun exists in a barren or starless portion of space. For example, in a region of 120 billions of miles in diameter four stars only, three of which are lucid, are found; beyond this space the more lucid stars are arranged as a ring, nearly, but not quite, in the same visual direction as the Galaxy. In addition, there are many lucid stars scattered over the heavens apparently unassociated with the Milky Way. Now does the starless interval of which Mr. Proctor speaks exist between the lucid stars and the Galaxy? or is it the barren space in which the sun is situated? Mr. Proctor's remarks on p. 86 are opposed to the idea of a starless space between the lucid stars and the Milky Way, the brighter stars drawing the smaller stars of the Galaxy towards them.

An important feature of the Galaxy consists of the large number of starry clusters found within it. The projection by Sidney Waters of the Nebulæ and Clusters in plate 7 exhibits this feature in the most striking manner, as but few clusters are found in other parts of the heavens. It was the elder Herschel who first drew attention to this feature of the Milky Way. "The stars," said Sir William, "of which it is composed are very unequally scattered, and show evident marks of clustering together into many separate allotments." Alluding to his equal-surface charts of the brighter orders of stars as being the first step towards just views of the constitution of the Milky Way, Mr. Proctor mentions on p. 221 this view of Herschel as a previous one, and therefore, in his opinion, the first to lead us to form an accurate conception of the Galaxy. He speaks of it as "the great, but little noticed discovery of Sir William Herschel's, that the bright clouds of the Milky Way are for the most part spherical clusters of stars. The excess in the number of lucid stars in the Milky Way proves that the stars of our constellations are among the leading orbs of the spherical aggregations." To this statement of Mr. Proctor we would solicit particular attention. It appears remarkable that the "lucid stars of our constellations" should be the "leading orbs of the spherical aggregations" of the Milky Way. This Mr. Proctor regards as proved by "the excess of the number of lucid stars in the Milky Way;" but does the apparent superposition of the lucid stars on the visible area of the Milky Way really *prove* the connexion assumed by Mr. Proctor? We fail to trace on plate 8 that very close connexion between the lucid stars and clusters which is indicated by Mr. Proctor's statement; besides, he himself, on p. 9, shows that the two streams are not absolutely coincident, the lucid stars lying on one side of the Milky Way near Scorpio, crossing it in the neighbourhood of Crux, and passing to the other side along Canis major, Orion, and Taurus. What if the starless interval of which Mr. Proctor speaks should lie between these zones of lucid stars and the very much smaller stars of the Galaxy? In this case could these same lucid stars be the leading orbs of the spherical aggregations of the Milky Way? We think not.

If we find it difficult to conceive of the lucid stars being thus imbedded in the Galaxy, we find it still more difficult to regard the collection of nebulae which, in the neighbourhood of the pole of the Milky Way, constitutes the rich region of Virgo, as forming part of the Galaxy. Messrs. Proctor and Waters could not have furnished us with more important information as to the distribution of lucid stars, star-clusters, and the apparent relation of these orders to the Milky Way than they have done in the beautiful charts which illustrate their views; but when Mr. Proctor tells us that the nebulae are, as well as the lucid stars, portions of this same wonderful zone, we must simply say we fail to see such a relation. We can understand that the starry cluster, of which our sun is a component member, is bounded, at any rate apparently, by a zone or ring of lucid stars, the plane of which intersects the plane of that

zone of smaller stars which appears to bound our view of the heavens when we look towards the Galaxy; and we can also understand that these two collections of larger and smaller stars may be arranged as two comparatively thin disks—one in which the larger stars are scattered (more or less congregated or otherwise) over its area, the other in which the smaller stars (those constituting the Milky Way) are congregated towards its edges; but we cannot understand how large collections of nebulae forming entirely a different class from the two before mentioned can exist above and below these planes, and at the same time form intimate portions of the far distant zone of small stars; indeed the zone in which nebulae are very sparsely distributed so distinctly separates the region of these bodies from those of the larger and smaller stars, as shown in the charts, that it is a matter of surprise that they should ever have been associated together. If, however, Mr. Proctor considers that the four orders of aggregation mentioned by him constitute “the universe,” we fully agree with him; but that universe must, from his own showing of the distribution of nebulae, greatly exceed the dimensions of the Milky Way properly so called.

Passing from the consideration of the three great phenomena of the starry heavens, the Milky Way, with its clustering aggregations of stars of inferior magnitude and brilliancy, the regular and irregular nebulae, and the zone of lucid stars, we proceed to examine the views set forth by Mr. Proctor under the title of “Star Streams.” This is the subject of his opening Essays; and in them he endeavours to show that all the brighter stars are arranged in streams, adducing in the second Essay the connecting-band of Pisces and other collections of stars of nearly the same order of brightness as examples. If these are considered in connexion with the foregoing classes of phenomena, it becomes apparent that our author recognizes (although it may be difficult for him to enunciate its nature) the existence of a force that draws the stars together, not into the spherical aggregations suggested by Sir William Herschel, but into long narrow thin streams, and this alike in every part of the heavens reached by our telescopes. The wisps and sprays characterizing as well the borders of the Milky Way as the convolutions and spires of the irregular nebulae, the great stream of lucid stars encircling the heavens, and the streams of various extent seen in our nocturnal skies, are alike phenomena of the same class, and betoken, should their existence be established, the operation of a force differing, it would seem, from that great and mighty force to which the spherical aggregations of Sir William Herschel are due; and it becomes an important point to ascertain if the star-streams advocated by Mr. Proctor are only apparent or have a real existence.

In the consideration of the evidence of the actual and real existence of star-streams it is necessary to take *distance* into account. Apparent star-streams, according to Mr. Proctor’s own showing, extend over vast spaces of the hollow celestial sphere *as recognized*

by the eye. Now we have *evidence* from actual measurement that the stars α and β Centauri and 61 Cygni, with our sun, are certainly the *only* stars within a sphere of something like 120 billions of miles in diameter. Ought not the stars composing a stream to be sufficiently near each other to afford some degree of *similarity* of parallax? We can easily understand that a group similar to the Pleiades may be physically connected. From double-star measurements we are certain of the physical connexion of a large number of pairs, and also of many triple combinations, without calling in the aid of the theory of probabilities to "absolutely demonstrate" such a connexion; and from the certainty thus afforded by measurements we may pass on by analogy to *infer* that the larger star-clusters, as for example the Pleiades, are also physically connected. When, however, we extend this analogy to "star-streams," the first point that strikes our attention is the great *distances* at which the component stars of the stream must be from each other. Any two stars situated on the connecting band of Pisces (one of Mr. Proctor's illustrations) at an angular distance of 1 degree would, with a parallax equal to that of 61 Cygni, be more than a billion of miles apart; and as we are ignorant of their parallax, they may, for aught we know, be much further asunder. Consequently each portion of the double star-stream of Pisces, if it have a real existence, must be very long; in fact, if the stars be not optically near each other, they must be at pretty nearly equal distances from our sun, and would, of course, present nearly similar parallaxes.

We have been particularly struck with the constant appeal by Mr. Proctor, on the one hand, to the application of the theory of probabilities to substantiate his views, and, on the other, the reiterated improbability that the present arrangement of the components of the starry heavens is a *chance* arrangement. As regards the latter, we cannot for a moment conceive of an astronomer holding such a view. Upon reading Mr. Proctor's argument against chance distribution, and examining his illustrations from Argelander and Chacornac on pp. 30, 31, 33, and 34, we took in the palm of our hand a pinch of fine sand (the silver sand of commerce); and having got rid of the superfluous grains by depositing them in the other palm, so as to leave on the hand a mere dust of sand, we sprinkled this dust lightly on a black japanned tea-tray. Having done this a few times we obtained a fine collection of clustered grains, a miniature cluster in Perseus for example, and specimens as fine as any engraved by Mr. Proctor, of star-streams and star-sprays, which were finely seen by the aid of an ordinary reading-lens. Now the mode of production of these clusters, streams, and sprays of sand would by most persons be attributed to "chance;" but such is not the case, as shown by Mr. Proctor on p. 100. It was altogether out of our power to produce any specified form on the tray; yet we cannot doubt the presence of "forces" so operating as to give to each grain of sand the exact position it occupied on the tray. Every feature insisted on by Mr. Proctor was there; the very vacant spaces which he considers so significant were seen; and although

in this experiment the clustering, the streaming, and the production of vacant spaces were not the result of the force of gravity acting among the particles themselves so as to occasion these clusterings and segregations, yet this same force, combined with the muscular force which determined the passage of the grains from the hand, determined and fixed for the time being the position of each individual grain on the tray. The assumption of stars of uniform size being distributed at uniform distances in space cannot be regarded in any sense as a "chance" distribution; for uniformity of such a character implies design, a design manifestly absent in the simple experiment above named. Mr. Proctor's star-streams are but the results of forces operating on and among the stellar orbs, the principal of which must doubtless be gravity; but how star-streams are produced we have yet to learn.

The remaining subject in Mr. Proctor's book is "star-drift;" and here we feel we are upon much more substantial ground than when engaged in the examination of the other portions of his work. His views of "star-drift" are based, not on conjecture, but upon observation. His two charts of the proper motions of stars form a valuable contribution to the records of observational astronomy; and we venture to predict that, in pursuing the course opened up by Mr. Proctor, astronomers will become much better acquainted with the distribution of the stars in space than by any other method whatever. We would, however, call attention to that which we consider to be a great defect in a work specially treating on the "universe." It is the absence of any, except the most incidental, mention of the binary and multiple systems. Surely, if Mr. Proctor desires to place the important subject of "star-drift" on an imperishable basis, it must be by showing that the drift-systems of Taurus, Cancer, and Ursa Major partake of the nature of multiple systems; and that the reader may be able to form a clear conception of this link in the great systematic chain, it is essential that he be led upwards step by step by the careful study of the binary systems. It may be said that Mr. Proctor has provided for this in some of his other works; but it is this fragmentary character of some of his writings, the work before us especially, that we object to. We should have perused it with much greater interest had it been arranged differently. And while upon this point, we would just say that an index is an essential part of a work like the present; there are many portions of it to which reference may often be necessary, and which may be difficult to find without such a help.

The restriction of the term "star-drift" to the motions figured to the eye on p. 141, as distinguished from that which may be designated "sun-drift," is a happy one; and we cannot close this notice of the first part of the volume without congratulating Mr. Proctor on a discovery by which that of binary systems by the elder Herschel is extended to portions of the starry universe containing systems of a much more imposing character. Mr. Proctor repudiates the principle, so far as he is personally concerned, of examining detail; but, if we mistake not, his most successful result has

emanated from the uninviting work of examining in detail the stellar proper motions.

On the coming transits of Venus we have but little, if any thing, to say. To us this portion of the volume appears to assume too much of personality; and we therefore leave the matter for its final settlement in December next. The plates, however, illustrating it may be consulted with great advantage.

Elementary Astronomy, or Notes and Questions on the Stars and Solar System for the use of Schools. By C. C. REEKS. London: Van Voorst. 1873.

Although this little book of fifty pages is intended for young people and even children, it contains information which in our opinion many "children of a larger growth" ought to become acquainted with. The author has given in it an interesting *résumé* of all the salient points of the physical characteristics of the stars, nebulae, planets, comets, meteors, and especially of the sun, divested of matter which too often tends to "make a book" without really illustrating the subject in hand. Each paragraph brings before the reader in condensed language an astronomical (and, as regards the earth, a geological) fact worth remembering. It is the simplicity and perspicuous brevity which characterizes this epitome of astronomy that has induced us to notice it in our pages as a work that should find a corner in every library.

XLVIII. *Proceedings of Learned Societies.*

ROYAL INSTITUTION OF GREAT BRITAIN.

Jan. 16, "ON the Acoustic Transparency and Opacity of the Atmosphere." By Professor Tyndall, D.C.L., LL.D., F.R.S. 1874.

This discourse embraced the scientific results of an inquiry on fog-signals, undertaken at the instance of the Elder Brethren of the Trinity House, and communicated, with their friendly concurrence, to the Royal Society and the Royal Institution.

The investigation was begun on the 19th of May, 1873, and continued till the 4th of July. It was resumed on the 8th of October, and continued to the end of November. It also includes observations made during the dense fog which enveloped London on December 9 and the succeeding days.

Gongs and bells were excluded from this investigation, in consequence of their proved inferiority to other instruments of signalling. The experiments were made with trumpets blown by powerfully compressed air, with steam-whistles, guns, and a steam syren associated with a trumpet 16 feet long.

Daboll's horn, or trumpet, had been highly spoken of by writers on fog-signals. A third-order apparatus of the kind had been reported as sending its sound to a distance of from 7 to 9 miles against the wind, and to a distance of 12 to 14 miles with the wind.

Holmes had improved upon Daboll; and with two instruments of Holmes, not of the third but of the first order, our experiments were made. On the 19th of May, at 3 miles distance, they became useless as a fog-signal; at a distance of 4 miles, with paddles stopped and all on board quiet, they were wholly unheard. At a distance of two miles from the Foreland the whistles tested on May 19 became useless. The 12-o'clock gun, fired with a 1-lb. charge at Drop Fort in Dover, was well heard on May 19, when the horns and whistles were inaudible. On the 20th of May the permeability of the atmosphere had somewhat increased, but the steam-whistle failed to pierce it to a depth of 3 miles. At 4 miles the horns, though aided by quietness on board, were barely heard. By careful nursing, if I may use the expression, the horn-sounds were carried to a distance of 6 miles. The superiority of the 18-pounder gun, already employed by the Trinity House as a fog-signal, over horns and whistles was on this day so decided as almost to warrant its recommendation to the exclusion of all the other signals.

Nothing occurred on the 2nd of June to exalt our hopes of the trumpets and whistles. The horns were scarcely heard at a distance of 3 miles; sometimes indeed they failed to be heard at 2 miles. By keeping every thing quiet on board, they were afterwards heard to a distance of 6 miles—a result, however, mainly due to the improved condition of the atmosphere. Considering the demands as to sound-range made by writers on this subject, the demonstrated incompetence of horns and whistles of great reputed power to meet these demands was not encouraging. On the 3rd of June the atmosphere had changed surprisingly. It was loaded overhead with dark and threatening clouds; the sounds, nevertheless, were well heard beyond 9 miles. On June 10 the acoustic transparency of the air was also very fair, the distance penetrated being upwards of $8\frac{3}{4}$ miles. The subsidence of the sound near the boundary of the acoustic shadow on the Dover side of the Foreland, and before entering the shadow, was to-day sudden and extraordinary, affecting equally both horns and guns. We were warned on June 3 that the supremacy of the gun on one occasion by no means implied its supremacy on all occasions, the self-same guns which on May 20 had so far transcended the horns being on this day their equals and nothing more. The 11th of June was employed in mastering still further the facts relating to the subsidence of the sound east and west of the Foreland—the cause of this subsidence being in part due to the weakening of the sonorous waves by their divergence into the sound-shadow, and in part to interference.

The atmosphere on the 25th of June was again very defective acoustically. The sounds reached a maximum distance of $6\frac{1}{2}$ miles; but at 4 miles, on returning from the maximum distance, they were very faint. The day had, as it were, darkened acoustically. On this day the guns lost still further their preeminence, and at $5\frac{1}{2}$ miles were inferior to the horn. No sounds whatever reached Dover Pier on the 11th; and it was only towards the close of the day that they succeeded in reaching it on the 25th. Thus by slow

degrees the caprices of the atmosphere made themselves known to us, showing us subsequently that within the limits of a single day, even within a single minute, the air, as a vehicle of sound, underwent most serious variations. The 26th of June was a far better day than its predecessor, the acoustic range being over $9\frac{1}{4}$ miles. The direction of the wind was less favourable to the sound on this day than on the preceding one, plainly proving that something else than the wind must play an important part in shortening the sound-range.

On the 1st of July we experimented upon a rotating horn, and heard its direct or axial blast, which proved to be the strongest, at a distance of $10\frac{1}{2}$ miles. The sounds were also heard at the Varne light-ship, which is $12\frac{3}{4}$ miles from the Foreland. The atmosphere had become decidedly clear acoustically, but not so optically; for on this day thick haze obscured the white cliffs of the Foreland. In fact, on days of far greater optical purity, the sound had failed to reach one third of the distance attained to-day. In the light of such a fact, any attempt to make optical transparency a measure of acoustic transparency must be seen to be delusive. On the 1st of July a 12-inch American whistle, of which we had heard a highly favourable account, was tried in place of the 12-inch English whistle; but, like its predecessor, the performance of the new instrument fell behind that of the horns. An interval of twelve hours sufficed to convert the acoustically clear atmosphere of July 1 into an opaque one; for on the 2nd of July even the horn-sounds, with paddles stopped and all noiseless on board, could not penetrate further than 4 miles.

Thus each succeeding day provided us with a virtually new atmosphere, clearly showing that conclusions founded upon one day's observations might utterly break down in the presence of the phenomena of another day. This was most impressively demonstrated on the day now to be referred to. The acoustic imperviousness of the 3rd of July was found to be still greater than that of the 2nd, while the optical purity of the day was sensibly perfect. The cliffs of the Foreland could be seen to-day at ten times the distance at which they ceased to be visible on the 1st, while the sounds were cut off at one sixth of the distance. At 2 P.M. neither guns nor trumpets were able to pierce the transparent air to a depth of 3, hardly to a depth of 2 miles. This extraordinary opacity was proved conclusively to arise from the irregular admixture with the air of the aqueous vapour raised by a powerful sun. This vapour, though perfectly invisible, produced an *acoustic cloud* impervious to the sound, and from which the sound-waves were thrown back as the waves of light are from an ordinary cloud. The waves thus refused transmission produced by their reflection echoes of extraordinary strength and duration. This, I may remark, is the first time that audible echoes have been proved to be reflected from an optically transparent atmosphere. By the lowering of the sun the production of the vapour was checked, and the transmissive power of the atmosphere restored to such an extent that, at a distance of 2 miles

from the Foreland, at 7 P.M., the intensity of the sound was at least thirty-six times its intensity at 2 P.M.

That layers of dried air alternating with layers of air saturated with the vapour of a volatile liquid can powerfully intercept sound, has been experimentally established.

On October 8 the observations were resumed, a steam syren and a Canadian whistle of great power being added to the list of instruments. A boiler had its steam raised to a pressure of 70 lbs. to the square inch; on opening a valve this steam would issue forcibly in a continuous stream; and the sole function of the syren was to convert this stream into a series of separate strong puffs. This was done by causing a disk with twelve radial slits to rotate behind a fixed disk with the same number of slits. When the slits coincided a puff escaped; when they did not coincide the outflow of steam was interrupted. Each puff of steam at this high pressure generated a sonorous wave of great intensity, the successive waves linking themselves together to a musical sound so intense as to be best described as a continuous explosion.

During the earlier part of October 8 the optical transparency of the air was very great; its acoustic transparency, on the other hand, was very defective. Clouds blackened and broke into a rain-and hail-shower of tropical violence. The sounds, instead of being deadened, were improved by this furious squall; and after it had lightened, thus lessening the local noises, the sounds were heard at a distance of $7\frac{1}{2}$ miles distinctly louder than they had been heard through the preceding rainless atmosphere at a distance of 5 miles. At 5 miles distance, therefore, the intensity of the sound had been at least doubled by the rain—a result entirely opposed to all previous assertions, but an obvious consequence of the removal by condensation and precipitation of that vapour the mixture of which with the air had been proved so prejudicial to the sound. On this day a dependence was established between the pitch of a note and its penetrative power—the syren generating 480 waves being slightly inferior to the horns, while generating 400 waves a second it was distinctly superior. The maximum range on October 8 was 9 miles. On October 9 the transmissive power had diminished, the maximum range being $7\frac{1}{2}$ miles. On both these days the syren proved to be superior to the horns, and on some occasions superior to the gun.

On the 10th and 11th, a gale having caused our steamer to seek safety in the Downs, we made land-observations. The duration of the aerial echoes was for the syren and the gun 9 seconds, for the horns 6 seconds. The duration varies from day to day. We sought to estimate the influence of the violent wind, and found that the sound of the gun failed to reach us in two cases at a distance of 550 yards to windward, the sound of the syren at the same time rising to a piercing intensity. To leeward the gun was heard at five times, and certainly might have been heard at fifteen times the distance attained to windward. The momentary character of the gun-sound renders it liable to be quenched by a single puff of wind;

but sounds of low pitch generally, whether momentary or not, suffer more from an opposing wind than high ones. We had on the 13th another example of the powerlessness of heavy rain to deaden sound.

On the 14th the maximum range was 10 miles, but the atmosphere did not maintain this power of transmission. It was a day of extreme optical clearness; but its acoustic clearness diminished as the day advanced. In fact the sun was in action. We proved to-day that by lowering the pitch of the Canadian whistle its sound, which had previously been inaudible, became suddenly audible. The day at first was favourable to the transmission of the longer sound-waves. After a lapse of three hours the case was reversed, the high-pitched syren being then heard when both guns and horns were inaudible. But even this state of things did not continue, so rapid and surprising are the caprices of the atmosphere. At a distance of 5 miles, at 3.30 P.M., the change in transmissive power reduced the intensity of the sound to at least one half of what it possessed at 11.30 A.M., the wind throughout maintaining the same strength and direction. Through all this complexity the knowledge obtained on July 3 sheds the light of a principle which reduces to order the apparent confusion.

October 15 was spent at Dungeness in examining the performance of Daboll's horn. It is a fine instrument, and its application was ably worked out by its inventor; still it would require very favourable atmospheric conditions to enable it to warn a steamer before she had come dangerously close to the shore. The direction in which the aerial echoes return was finely illustrated to-day, that direction being always the one in which the axis of the horn is pointed.

The 16th was a day of exceeding optical transparency, but of great acoustic opacity. The maximum range was only 5 miles. On this day the howitzer and all the whistles were clearly overmastered by the syren. It was moreover heard at $3\frac{1}{2}$ miles with the paddles going, while the gun was unheard at $2\frac{1}{2}$ miles. With no visible object that could possibly yield an echo in sight, the pure aerial echoes, coming from the more distant southern air, were distinct and long continued at a distance of 2 miles from the shore. Near the base of the Foreland cliff we determined their duration, and found it to be 11 seconds, while that of the best whistle-echoes was 6 seconds. On this day three whistles, sounded simultaneously, were pitted against the syren, and found clearly inferior to it.

On the 17th four horns were compared with the syren and found inferior to it. This was our day of greatest acoustic transparency, the sound reaching a maximum of 15 miles for the syren, and of more than 16 for the gun. The echoes on this day were continued longer than on any other occasion. They continued for 15 seconds, their duration indicating the atmospheric *depth* from which they came.

On October 18, though the experiments were not directed to determine the transmissive power of the air, we were not without

proof that it continued to be high. From 10 to 10.30 A.M., while waiting for the blasts of the syren at a distance of 3 miles from the Foreland, the continued reports of what we supposed to be the musketry of skirmishing parties on land were distinctly heard by us all. We afterwards learned that the sounds arose from the rifle-practice on Kingsdown beach, $5\frac{1}{2}$ miles away. On July 3, which, optically considered, was a far more perfect day, the 18-pounder howitzer and mortar failed to make themselves heard at half this distance. The 18th was mainly occupied in determining the influence of pitch and pressure on the syren-sound. Taking the fluctuations of the atmosphere into account, I am of opinion that the syren, performing from 2000 to 2400 revolutions a second, or, in other words, generating from 400 to 480 waves per second, best meets the atmospheric conditions. We varied the pressure from 40 to 80 lbs. on the square inch; and though the intensity did not appear to rise in proportion to the pressure, the higher pressure yielded the hardest and most penetrating sound.

The 20th was a rainy day with strong wind. Up to a distance of $5\frac{1}{2}$ miles the syren continued to be heard through the sea- and paddle-noises. In rough weather, indeed, when local noises interfere, the syren-sound far transcends all other sounds. On various occasions to-day it proved its mastery over both gun and horns. On the 21st the wind was strong and the sea high. The horn-sounds, with paddles going, were lost at 4 miles, while the syren continued serviceable up to $6\frac{1}{2}$ miles. The gun to-day was completely overmastered. Its puffs were seen at the Foreland; but its sound was unheard when the syren was distinctly heard. Heavy rain failed to damp the power of the syren. The whistles were also tried to-day, but were found far inferior to the syren. On the 22nd it blew a gale, and the 'Galatea' quitted us. We made observations on land on the influence of the wind and of local noises. The shelter of the Coastguard Station at Cornhill enabled us to hear gun-sounds which were quite inaudible to an observer out of shelter; in the shelter also both horn and syren rose distinctly in power; but they were also heard outside when the gun was quite unheard. As usual, the sound to leeward was far more powerful than those at equal distances to windward. The echoes from the cloudless air were to-day very fine. On the 23rd, in the absence of the steamer, the observations on the influence of the wind were continued. The quenching of the gun-sounds in particular to windward was well illustrated. All the sounds, however, gun included, were carried much further to leeward than to windward. The effect of a violent thunderstorm and downpour of rain in exalting the sound was noticed by observers both to windward and to leeward of the Foreland. In the rear of the syren its range to-day was about a mile. At right angles to the axis, and to windward, it was about the same. To leeward it reached a distance of $7\frac{1}{2}$ miles.

On the 24th, when observations were made afloat in the steam-tug 'Palmerston,' the syren exhibited a clear mastery over gun and

horns. The maximum range was $7\frac{3}{4}$ miles. The wind had changed from W.S.W. to S.E., then to E. As a consequence of this, the syren was heard loudly in the streets of Dover. On the 27th the wind was E.N.E., and the syren-sound penetrated everywhere through Dover, rising over the moaning of the wind and all other noises. It was heard at a distance of 6 miles from the Foreland on the road to Folkestone, and would probably have been heard all the way to Folkestone had not the experiments ceased. Afloat and in the axis, with a high wind and sea, the syren, and it only, reached to a distance of 6 miles; at 5 miles it was heard through the paddle-noises. On the 28th further experiments were made on the influence of pitch, the syren when generating 480 waves a second being found more effective than when generating 300 waves a second. The maximum range in the axis on this day was $7\frac{1}{2}$ miles.

The 29th of October was a day of extraordinary optical transparency, but by no means transparent acoustically. The gun was the greatest sufferer. At first it was barely heard at 5 miles; but afterwards it was tried at $5\frac{1}{2}$, $4\frac{1}{2}$, and $2\frac{1}{2}$ miles, and was heard at none of these distances. The syren at the same time was distinctly heard. The sun was shining strongly; and to its augmenting power the enfeeblement of the gun-sound was doubtless due. At $3\frac{1}{2}$ miles, subsequently, dead to windward, the syren was faintly heard; the gun was unheard at $2\frac{3}{4}$ miles. On land the syren and horn-sounds were heard to windward at 2 to $2\frac{1}{2}$ miles, to leeward at 7 miles; while in the rear of the instruments they were heard at a distance of 5 miles, or five times as far as they had been heard on October 23.

The 30th of October furnished another illustration of the fallacy of the notion which considers optical and acoustic transparency to go hand in hand. The day was very hazy, the white cliffs of the Foreland at the greater distances being quite hidden; still the gun- and syren-sounds reached, on the bearing of the Varne light-vessel, to a distance of $11\frac{1}{2}$ miles. The syren was heard through the paddle-noises at $9\frac{1}{4}$ miles, while at $8\frac{1}{2}$ miles it became efficient as a signal with the paddles going. The horns were heard at $6\frac{1}{4}$ miles. This was during calm. Subsequently, with a wind from the N.N.W., no sounds were heard at $6\frac{1}{2}$ miles. On land, the wind being across the direction of the sound, the syren was heard only to a distance of 3 miles N.E. of the Foreland; in the other direction it was heard plainly on Folkestone Pier, 8 miles distant. Both gun and horns failed to reach Folkestone.

Wind, rain, a rough sea, and great acoustic opacity characterized October 31. Both gun and horns were unheard 3 miles away, the syren at the same time being clearly heard. It afterwards forced its sound with great power through a violent rain-squall. Wishing the same individual judgment to be brought to bear upon the sounds on both sides of the Foreland, in the absence of our steamer, which had quitted us for safety, I committed the observations to Mr. Douglass. He heard them at 2 miles on the

Dover side; and on the Sandwich side, with the same intensity, at 6 miles.

A gap (employed by the engineers in making arrangements for pointing the syren in any required direction) here occurred in our observations. They were resumed, however, on November 21, when comparative experiments were made upon the gun and syren. Both sources of sound, when employed as fog-signals, will not unfrequently have to cover an arc of 180° ; and it was desirable to know with greater precision how the sound is affected by the direction in which the gun or syren is pointed.

The gun, therefore, was in the first instance pointed on us and fired, then turned and fired along a line perpendicular to that joining us and it. There was a sensible, though small, difference between the sounds which reached us in the two cases. A similar experiment was made with the syren; and here the falling off when the instrument was pointed perpendicular to the line joining us and it was very considerable. This is what is to be expected; for the trumpet associated with the syren is expressly intended to gather up the sound and project it in a certain direction, while no such object is in view in the construction of the gun. The experiments here referred to were amply corroborated by others made on November 22 and 23.

On both of these days the 'Galatea's' guns were fired to windward and to leeward. The aerial echoes in the latter case were distinctly louder and longer than in the former. The experiment has been repeated many times, and always with the same result.

In front of the Cornhill Coastguard Station, and only $1\frac{1}{4}$ mile from the Foreland, the syren, on the 21st, though pointed towards us, fell suddenly and considerably in power. Before reaching Dover Pier it had ceased to be heard. The wind was here against the sound; but this, though it contributed to the effect, could not account for it; nor could the proximity of the shadow account for it. To these two causes must have been added an acoustically flocculent, though optically transparent, atmosphere. The experiment demonstrates conclusively that there are atmospheric and local conditions which, when combined, prevent our most powerful instruments from making more than a distant approach to the performance which writers on fog-signals have demanded of them.

On November 24 the sound of the syren, pointed to windward, was compared at equal distances in front of and behind the instrument. It was louder to leeward in the rear than at equal distances to windward in front. Hence in a wind the desirability of pointing the instrument to windward. The whistles were tested this day in comparison with the syren deprived of its trumpet. The Canadian and the 8-inch whistles proved the most effective; but the naked syren was as well heard as either of them. As regards opacity, the 25th of November almost rivalled the 3rd of July. The gun failed to be heard at a distance of 2.8 miles, and it yielded only a faint crack at $2\frac{1}{2}$ miles.

Meanwhile this investigation has given us a knowledge of the atmosphere in its relation to sound, of which no notion had been previously entertained. While the *velocity* of sound has been the subject of refined and repeated experiments, I am not aware that, since the publication of a celebrated paper by Dr. Derham in the 'Philosophical Transactions' for 1708, any systematic inquiry has been made into the causes which affect the *intensity* of sound in the atmosphere. Derham's results, though obtained at a time when the means of investigation were very defective, have apparently been accepted with unquestioning trust by all subsequent writers—a fact which is, I think, in some part to be ascribed to the *à priori* probability of his conclusions.

Thus Dr. Robinson, relying apparently upon Derham, says, "Fog is a powerful damper of sound," and he gives us physical reason why it must be so. "It is a mixture of air and globules of water, and at each of the innumerable surfaces where these two touch, a portion of the vibration is reflected and lost." And he adds further on, "The remarkable power of fogs to deaden the report of guns has been often noticed."

Assuming it, moreover, as probable that the measure of "a fog's power in stopping sound" bears some simple relation to its opacity for light, Dr. Robinson, adopting a suggestion of Mr. Alexander Cunningham, states that "the distance at which a given object, say, a flag or pole, disappears may be taken as a measure of the fog's power" to obstruct the sound. This is quite in accordance with prevalent notions; and granting that the sound is dissipated, as assumed, by reflection from the particles of fog, the conclusion follows that the greater the number of the reflecting particles, the greater will be the waste of sound. But the number of particles, or, in other words, the density of the fog, is declared by its action upon light; hence the optical opacity will be a measure of the acoustic opacity.

This, I say, expresses the opinion generally entertained, "clear still air" being regarded as the best vehicle for sound. We have not, as stated above, experimented in really dense fogs; but the experiments actually made entirely destroy the notion that clear weather is necessarily better for the transmission of sound than thick weather. Some of our days of densest acoustic opacity have been marvellously clear optically, while some of our days of thick haze have shown themselves highly favourable to the transmission of sound. Were the physical cause of the sound-waste that above assigned, did that waste arise in any material degree from reflection at the limiting surfaces of the particles of haze, this result would be inexplicable.

Again, Derham, as quoted by Sir John Herschel, says that "falling rain tends powerfully to obstruct sound." We have had repeated reversals of this conclusion. Some of our observations have been made on days when rain and hail descended with a perfectly tropical fury; and in no single case did the rain deaden the sound; in every case, indeed, it had precisely the opposite effect.

But falling snow, according to Derham, offers a more serious obstacle than any other meteorological agent to the transmission of sound. We have not extended our observations at the South Foreland into snowy weather; but an observation of my own made on December 29, in the Alps, during a heavy snowstorm, distinctly negatives the statement of Derham.

Reverting to the case of fog, I am unable in modern observations to discover any thing conclusive as to its alleged power of deadening sound. I had the pleasure of listening to a very interesting lecture on fog-signals delivered by Mr. Beazeley before the United-Service Institution; and I have carefully perused the printed report of that lecture, and of a paper previously communicated by Mr. Beazeley to the Institution of Civil Engineers. But in neither of these painstaking compilations can I find any adequate evidence of the alleged power of fogs to deaden sound.

Indeed, during the discussion which followed the reading of Mr. Beazeley's paper, an important observation in an opposite sense was mentioned by Mr. Douglass, to whose ability and accuracy as an observer I am able to bear the strongest testimony. Mr. Douglass stated that he had found in his experience but little difference in the travelling of sound in foggy or in clear weather. He had distinctly heard in a fog, at the Smalls rock in the Bristol Channel, guns fired at Milford Haven 25 miles away. Mr. Beazeley, moreover, has heard the Lundy-Island gun "at Hartland Point," a distance of 10 miles, during dense fog. Mr. Beazeley's conclusion, indeed, accurately expresses the state of our knowledge when he wrote. In winding up his paper he admitted "that the subject appeared to be very little known, and that the more it was looked into the more apparent became the fact that the evidence as to the effect of fog upon sound is extremely conflicting." When, therefore, it is alleged, as it is so often alleged, that the power of fogs to deaden sound is well known, the disjunctive not is to be inserted before the predicate.

The real enemy to the transmission of sound through the atmosphere has, I think, been clearly revealed by the foregoing inquiry. That enemy has been proved to be not rain, nor hail, nor haze, nor fog, nor snow—not water in fact in either a liquid or a solid form, but water in a vaporous form, mingled with air so as to render it acoustically turbid and flocculent. This acoustic turbidity often occurs on days of surprising optical transparency. Any system of measures, therefore, founded on the assumption that the optic and acoustic transparency of the atmosphere go hand in hand must prove delusive.

There is but one solution of this difficulty: it is to make the source of sound so powerful as to be able to endure loss by partial reflection and still retain a sufficient residue for transmission. Of all the instruments hitherto examined by us, the syren comes nearest to the fulfilment of this condition; and its establishment upon our coast will, in my opinion, prove an incalculable boon to the mariner.

An account of the observations made during the recent fog adds the force of demonstration to others recorded in the paper, that fogs possess no such power of stifling sound as that hitherto ascribed to them. Indeed the melting away of fog on December 13 was accompanied by an acoustic darkening of the atmosphere so great that at a point midway between the eastern end of the Serpentine, where a whistle was sounded, and the bridge, the sound possessed less than one fourth of the intensity which it possessed on the day of densest fog.

Thus, I think, has been removed the last of a congeries of errors which for more than a century and a half have been associated with the transmission of sound by the atmosphere.

ROYAL SOCIETY.

[Continued from p. 312.]

Nov. 27, 1873.—W. Spottiswoode, M.A., Treasurer and Vice-President, in the Chair.

The following communication was read :—

“Researches in Spectrum-Analysis in connexion with the Spectrum of the Sun.”—Part III. By J. Norman Lockyer.

The paper commences with an introduction, in which the general line of work since the last paper is indicated. Roughly speaking, this has been to ascertain the capabilities of the new method in a quantitative direction. It is stated that while qualitative spectrum-analysis depends upon the *positions* of the lines, quantitative spectrum-analysis on the other hand depends not on position but on the *length*, *brightness*, and *thickness* of the lines.

The necessity of maps carefully executed and showing the individuality of each line is shown ; and it is stated that the execution of these maps required the use of the electric arc to render the vapours of the metals incandescent. A battery of 30 Grove's cells of one pint capacity was accordingly employed in the researches about to be described.

The difficulties of eye-observations of the characters of the lines compelled the application of photography, another reason for the use of which existed in the facility it afforded for confronting spectra with each other, and so eliminating coincident lines, since the lines, if due to impurities, would be longest and thickest in the spectrum to which they really belonged.

The portion of the spectrum at present worked upon is that from H to F.

Another branch of the research has been the construction of a Table of all the named Fraunhofer lines, showing the lengths and thicknesses of the metallic lines to the absorption of which they were due ; this Table enabled the author to allocate upwards of 50 lines in the solar spectrum, presumably overlooked by Ångström and Thalén. The Table was intended as a preliminary to a new photographic map of the spectrum from H to F, on a larger scale than

Ångström's, which was intended to clear away all the difficulties touching coincidences—and to have below it complete maps of all the solar elements with their long and short lines. This map is incomplete at present, but is making rapid progress.

A preliminary search for elements supposed not to be in the sun has also been commenced.

Of the above-named researches the subsequent parts of the paper refer to:—

I. The experiments made on a possible quantitative spectrum-analysis.

II. The method of photographing spectra adopted.

III. The coincidences of spectrum lines.

IV. The preliminary inquiry into the existence in the sun of elements not previously traced.

I. The experiments made on a possible quantitative Spectrum-Analysis.

After the two former papers were sent in to the Royal Society, an investigation of the general changes undergone by spectra given by alloys was commenced.

A micrometer eyepiece was mounted on the observing-telescope of the spectroscope. With this the following phenomena were observed:—

I. The lines which remained varied their length as the percentage of the elements to which they were due varied.

II. Some of the lines appreciably varied their thickness or brightness, or both, in the same way.

III. In cases where the brightness of a line was estimated through a considerable range of percentage composition by comparison with an air-line, the air-line was observed to grow faint and then disappear as the brightness of the metallic lines increased.

IV. In cases where the brightness or thickness of the line of one element was estimated by comparison with the line adjacent of the other constituent of the alloy, the point of equal brightness was observed to ascend or descend; this method was used to avoid the uncertainty of micrometric measurements of the tips of the lines in consequence of their variation in length due to the unequal action of the spark.

V. In some cases, where the percentage of a constituent was so small that none of its lines were visible, there yet seemed to be an effect produced on the vapour of the opposite pole.

As these conclusions were derived from coarse alloys, and it was desirable to observe the effect of very fine gradation, Mr. C. Fremantle, the Deputy Master of the Mint, was begged to allow observations to be made on the gold-copper and silver-copper coinage alloys; and he immediately responded most courteously to the request.

Examples of the behaviour of some coarse alloys of silver and lead are given; they were irregular in their action; but it was ob-

served that silver lines remained in the alloy as long as from .05 to .02 per cent. of silver was present. The alloys, however, were very unequal. Experiments on cadmium and tin alloys are described, the cadmium forming 10, 5, 1.0, 0.15 per cent. In the last but one cadmium line was permanent; in the first at least five were seen. In an alloy of 0.099 per cent. of cadmium with a mixture of lead, tin, and zinc constituting the rest of the alloy, the behaviour of the cadmium lines was sensibly the same as in a mixture of 0.1 per cent. of cadmium and 99.9 of tin.

In the Mint specimens the same phenomena were observed *en petit* as the coarser alloys showed *en grand*. In a gold-copper alloy $\frac{1}{1000}$ increase in the gold made the lines shorter, and a similar increase in the copper made them longer.

In the silver-copper alloy an increase of $\frac{1}{1000}$ in the silver lengthened the lines, a similar increase in the copper shortened them.

These phenomena can be explained by assuming such alloys to be different physical things, and that the spark acts upon the alloy as a whole as well as upon each vapour separately.

Thus, in these Mint alloys, copper is common to both, and their melting-points are:—

Gold . . . 1200° (Pouillet).

Copper . . 1200° to 1000°, the precise point not determined.

Silver . . 1000° (Pouillet).

The intermediate position of copper explains the different action on its lines of gold and silver.

II. *The Method of photographing Spectra adopted.*

A camera carrying a 5 × 5-inch plate and a 3-inch lens of 23 inches focus, replaced the observing-telescope of the spectroscope. The lens focused from 3900 to 4500 very fairly upon the plate. The beam passing through collimator and prisms was, as in Mr. Rutherford's researches, very small. As the electric arc in its usual vertical position gave all the lines from pole to pole, the lamp was placed on its side, and the arc used in a horizontal position, the slit being vertical. The dense core of the arc then gave all the short lines in the centre of the field, the longer ones extending beyond them on either side. In order to obtain a scale, it was resolved to photograph the solar spectrum immediately adjacent to the metallic spectrum under examination.

To effect this a portion of the slit was covered up while the solar spectrum passed through the free part, and then the part used for the solar spectrum was covered, while the formerly covered part was opened for the metallic spectrum. This was effected by a shutter, with an opening sliding in front of the slit; a diagram of its action and form is given.

The arrangement of the spectroscope, heliostat, &c. for obtaining the sun's light is described. The image of the sun was brought

to a focus between the poles of the lamp by an extra lens interposed between the lamp and the heliostat.

The use of the shutter enables us to compare either two or more spectra upon a single plate; or the solar spectrum may be compared with two metallic spectra, being made to occupy the position between the two.

III. *On the Lines coincident in different Spectra.*

The bearing of the former papers on the lengths of the lines of the elements is briefly recapitulated.

The examination of the various spectra of metals and alloys indicated the great impurity of most of the metals used, and suggested the possibility of the coincidences observed by Thalén and others being explained in the light of former work.

It is observed that coincidences are particularly numerous in the spectra of iron, titanium, and calcium, and that nearly every other solar metallic spectrum has one or more lines coincident with lines of the last element. These coincident lines are, as a rule, very variable in length and intensity in various specimens of the metals in which they occur, and are sometimes altogether absent.

One of the longest calcium lines, that at wave-length 4226·3, is also seen in the strontium spectrum as a line of medium length; and 4607·5, a very long line in strontium, appears in calcium as a short line. Another very long strontium line, 4215·3, is asserted by Thalén to be seen in calcium; but the author has never seen it till lately, and then only in a specimen of calcium known to contain strontium.

We have here, then, a case of coincident lines, in which the one that is long and bright in one spectrum is short and faint in the other, and a case of a line said to be coincident in two spectra being, though always visible in one, sometimes absent in the other of them, and only appearing in it when the two substances were mixed. The hypothesis of impurity at once explains the whole case, even without the third line, which renders the fact of mixture certain.

The longest lines of calcium occur in iron, cobalt, nickel, barium, strontium, &c.; and the longest lines of iron occur in calcium, strontium, barium, and other metals.

Other cases are adduced; and the following general statements are hazarded, with a premise that further inquiry may modify them.

1. If the coincident lines of the metals be considered, those cases are rare in which the lines are of the first order of length in all the spectra to which they are common: those cases are much more frequent in which they are long in one spectrum and shorter in the others.

2. As a rule, in the instances of those lines of iron, cobalt, nickel, chromium, and manganese which are coincident with lines of calcium, the calcium lines are long, while the lines as they

appear in the spectra of the other metals are shorter than the longest lines of those metals. Hence we are justified in assuming that short lines of iron, cobalt, nickel, chromium, and manganese, coincident with long and strong lines of calcium, are really due to traces of the latter metal occurring in the former as an impurity.

3. In cases of coincidences of lines found between various spectra the line may be fairly assumed to belong to that one in which it is longest and brightest.

A description of some photographs of spectra is then given, a photograph of the coincident lines of calcium and strontium being amongst them, and proving that strontium occurs in the sun; and the section concludes with a brief description of the method employed in making the new map, showing lengths and thicknesses, and enumerating coincident lines. This is done thus: papers are pasted on to photographs of the solar spectrum on glass; the lengths of the lines of the metallic spectrum under examination (*e. g.* that of iron) are marked on this paper in prolongation of the solar lines to which they correspond. They are then copied upon a map; and another piece of paper being fixed down, another spectrum is proceeded with in the same way.

IV. *The preliminary inquiry into the existence of elements in the Sun not previously traced.*

The previous researches having shown that the former test for the presence or absence of a metal in the sun, namely the presence or absence of its brightest or strongest lines in the average solar spectrum, was not conclusive, a preliminary search for other metals was determined on; and as a guide, Mr. R. J. Friswell was requested to prepare two lists, showing broadly the chief chemical characteristics of the elements traced and not traced in the sun.

The Tables showed that, in the main, those metals which had been traced formed stable compounds with oxygen.

The author therefore determined to search for the metals which formed strong oxides, but which had not yet been traced.

The result up to the present time has been that *strontium*, *cadmium*, *lead*, *cerium*, and *uranium* would seem with considerable probability to exist in the solar reversing layer. Should the presence of *cerium* and *uranium* be subsequently confirmed, the whole of the iron group of metals will thus have been found in the sun.

Certain metals forming unstable oxides, such as gold, silver, mercury, &c. were sought for and not found. The same was the case when chlorine, bromine, iodine, &c. were sought by means of their lines produced in tubes by the jar-spark. These elements are distinguishable as a group by forming compounds with hydrogen.

It is observed that certain elementary and compound gases effect their principal absorption on the most refrangible part of the spectrum when they are rare, and that as they become dense the absorption approaches the less-refrangible end—that the spectra of compounds are banded or columnar, the bands or columns lying

at the red end of the spectrum—that the absorption spectra of chlorine, iodine, bromine, &c. are columnar, and that these are broken up by the spark just as the band spectra of compounds are broken up—and that it is probable that no compounds exist in the sun. The following facts, gathered from the work already accomplished by Rutherford and Secchi, are stated.

There are three classes of stars :—

1. Those like Sirius, the brightest (and therefore hottest?) star in the northern sky, their spectra showing only hydrogen lines very thick, and metallic lines exceedingly thin.

2. A class of stars with a spectrum differing only in degree from those of the class of Sirius; and to this our sun belongs.

3. A class of stars with columnar or banded spectra indicating the formation of compounds.

The question is asked whether all the above facts cannot be grouped together in a working hypothesis, which assumes that in the reversing layers of the sun and stars various degrees of “celestial dissociation” are at work which prevents the coming together of the atoms which, at the temperature of the earth, and at all artificial temperatures yet attained here, form the metals, the metalloids, and compounds.

In other words, the metalloids are regarded as *quasi* compound bodies when in the state in which we know them; and it is supposed that in the sun the temperature is too great to permit them to exist in that state in the reversing layer, though they may be found at the outer portions of the chromosphere or in the corona.

It is suggested that, if this hypothesis should gain strength from subsequent work, stony meteorites will represent the third class of metalloidal or compound stars, and iron meteorites the other or metallic stars.

The paper concludes as follows :—

“An interesting physical speculation connected with this working hypothesis is the effect on the period of duration of a star’s heat which would be brought about by assuming that the original atoms of which a star is composed are possessed of the increased potential energy of combination which this hypothesis endows them with. From the earliest phase of a star’s life the dissipation of energy would, as it were, bring into play a new supply of heat and so prolong the star’s life.

“May it not also be, if chemists take up this question, which has arisen from the spectroscopic evidence of what I have before termed the plasticity of the molecules of the metalloids taken as a whole, that much of the power of variation which is at present accorded to metals may be traced home to the metalloids? I need only refer to the fact that, so far as I can learn, all so-called changes of atomicity take place when metalloids are involved, and not when the metals alone are in question.

“As instances of these, I may refer to the triatomic combinations formed with chlorine, oxygen, sulphur, &c. in the case of

tetrad or hexad metals. May not this be explained by the plasticity of the metalloids in question?

“May we not from these ideas be justified in defining a metal, provisionally, as a substance the absorption-spectrum of which is generally the same as the radiation spectrum, while the metalloids are substances the absorption-spectrum of which, generally, is not the same?”

“In other words, in passing from a hot to a comparatively cold state, the plasticity of these latter comes into play, and we get a new molecular arrangement. Hence are we not justified in asking whether the change from oxygen to ozone is but a type of what takes place in all metalloids?”

Dec. 18.—Joseph Dalton Hooker, C.B., President, in the Chair.

The following communication was read :—

“On Double Refraction in a Viscous Fluid in motion. By J. Clerk Maxwell, M.A., F.R.S.

According to Poisson’s* theory of the internal friction of fluids, a viscous fluid behaves as an elastic solid would do if it were periodically liquefied for an instant and solidified again, so that at each fresh start it becomes for the moment like an elastic solid free from strain. The state of strain of certain transparent bodies may be investigated by means of their action on polarized light. This action was observed by Brewster, and was shown by Fresnel to be an instance of double refraction.

In 1866 I made some attempts to ascertain whether the state of strain in a viscous fluid in motion could be detected by its action on polarized light. I had a cylindrical box with a glass bottom. Within this box a solid cylinder could be made to rotate. The fluid to be examined was placed in the annular space between this cylinder and the sides of the box. Polarized light was thrown up through the fluid parallel to the axis, and the inner cylinder was then made to rotate. I was unable to obtain any result with solution of gum or sirup of sugar, though I observed an effect on polarized light when I compressed some Canada balsam which had become very thick and almost solid in a bottle.

It is easy, however, to observe the effect in Canada balsam, which is so fluid that it very rapidly assumes a level surface after being disturbed. Put some Canada balsam in a wide-mouthed square bottle; let light, polarized in a vertical plane, be transmitted through the fluid; observe the light through a Nicol’s prism, and turn the prism so as to cut off the light; insert a spatula in the Canada balsam, in a vertical plane passing through the eye. Whenever the spatula is moved up or down in the fluid, the light reappears on both sides of the spatula; this continues only so long as the spatula is in motion. As soon as the motion stops, the light disappears, and that so quickly that I have hitherto been unable to

* *Journal de l’École Polytechnique*, tome xiii. cah. xx. (1829).

determine the rate of relaxation of that state of strain which the light indicates.

If the motion of the spatula in its own plane, instead of being in the plane of polarization, is inclined 45° to it, no effect is observed, showing that the axes of strain are inclined 45° to the plane of shearing, as indicated by the theory.

I am not aware that this method of rendering visible the state of strain of a viscous fluid has been hitherto employed; but it appears capable of furnishing important information as to the nature of viscosity in different substances.

Among transparent solids there is considerable diversity in their action on polarized light. If a small portion is cut from a piece of unannealed glass at a place where the strain is uniform, the effect on polarized light vanishes as soon as the glass is relieved from the stress caused by the unequal contraction of the parts surrounding it.

But if a plate of gelatine is allowed to dry under longitudinal tension, a small piece cut out of it exhibits the same effect on light as it did before, showing that a state of strain can exist without the action of stress. A film of gutta percha which has been stretched in one direction has a similar action on light. If a circular piece is cut out of such a stretched film and warmed, it contracts in the direction in which the stretching took place.

The body of a sea-nettle has all the appearance of a transparent jelly; and at one time I thought that the spontaneous contractions of the living animal might be rendered visible by means of polarized light transmitted through its body. But I found that even a very considerable pressure applied to the sides of the sea-nettle produced no effect on polarized light, and I thus found, what I might have learned by dissection, that the sea-nettle is not a true jelly, but consists of cells filled with fluid.

On the other hand, the crystalline lens of the eye, as Brewster observed, has a strong action on polarized light when strained either by external pressure or by the unequal contraction of its parts as it becomes dry.

I have enumerated these instances of the application of polarized light to the study of the structure of solid bodies as suggestions with respect to the application of the same method to liquids so as to determine whether a given liquid differs from a solid in having a very small "rigidity," or in having a small "time of relaxation"*, or in both ways. Those which, like Canada balsam, act strongly on polarized light, have probably a small "rigidity," but a sensible "time of relaxation." Those which do not show this action are probably much more "rigid," and owe their fluidity to the smallness of their "time of relaxation."

* The "time of relaxation" of a substance strained in a given manner is the time required for the complete relaxation of the strain, supposing the rate of relaxation to remain the same as at the beginning of this time.

XLIX. *Intelligence and Miscellaneous Articles.*CALORIFIC EFFECTS OF THE MAGNETISM IN AN ELECTROMAGNET
WITH SEVERAL POLES. BY A. CAZIN.

I HAVE observed the calorific effects which accompany the disappearance of magnetism in the core of a rectilinear electromagnet presenting several *consequent points*, and have arrived at a very simple law:—

When a rectilinear core of iron is magnetized by a series of identical coils through which the current passes in alternately opposite directions, if the coils determine equal concamerations, the quantities of heat produced in the core by the disappearance of magnetism are inversely proportional to the squares of the numbers of concamerations.

The apparatus I used is a sort of differential air-thermometer, the reservoirs of which are formed by iron cylinders 42 centims. in length, 5 centims. in diameter, and about 2 millims. in thickness. The bases of these cylinders are closed by plates of copper. A glass tube having an internal diameter of 2 millims., bent in the shape of a U and containing water, unites the two cylinders: it serves as a manometer to measure the difference of pressure, H , which establishes itself between the two reservoirs when one of them is heated; and if it is only a question of relative thermic effects, we may content ourselves with observing this difference of pressure; it is proportional to the difference of temperature of the two reservoirs.

In order that these reservoirs may be in the same conditions in regard to their surroundings, each of them is enveloped in a coating of cotton and a tube of pasteboard; and then two are placed in the axis of two identical bobbins of wood without contact between the wood and the pasteboard. The copper wire which receives the current is coiled round one of the series of bobbins, and can magnetize the corresponding core. These precautions are necessary in order that the voltaic heat of the wire may not be transmitted to the core. I ascertained that this condition was satisfied: the current remaining closed for a long time, the water manometer indicated only an insignificant change.

To make an experiment, a discontinuous current is thrown into the magnetizing coils; a tell-tale registers the number n of the interruptions; at each minute the difference of the levels in the manometer is noted; and the noting is continued when the current is suppressed, until the initial levels are restored. A graphic sketch of these data makes known, on the one hand, the total effect H produced during the action of the discontinuous current, and, on the other, the correction h due to the cooling action of the surrounding bodies. Dividing $H + h$ by the number n of the interruptions, we have the effect of the disappearance of magnetism at each breaking of the voltaic circuit. This quantity serves as a relative measure for the heat generated by the magnetism.

The following is an example of this class of experiments.

The core is magnetized by four coils of 18 centims. external diameter and 8 centims. depth; the iron core extends 5 centims. beyond the extremities of the coils; the copper wire is 2 millims. in diameter.

The pile consists of eight rectangular Bunsen elements; and the intensity of the continuous current is 0.0218, the unit being that which disengages in a voltameter 1 milligramme of hydrogen per second.

In the following Table, by the side of the values of n is set down the corresponding duration in minutes, and the values H and h are millimetres of water.

No. of con- cations.	n .	min.	H .	h .	$\frac{H+h}{n}$	
					Observed.	Calculated.
4		3494 in 10	4.1	0	0.0011	0.0011
2		3100 „ 10	13.5	1.3	0.0047	0.0044
1		2003 „ 6	30.9	4.5	0.0177	0.0176

The experimental law of which the above is an exposition is conformable to the law of *magnetic energy* which I communicated to the Academy on Nov. 18, 1872. Let us designate by m the quantity of magnetism received by the core when the current traverses the four coils in the same direction without forming consequent points, by l the interval between the two poles; I have shown that the quantity of heat generated by the disappearance of this magnetism is measured by $m^2 l$.

Supposing that the current changes direction once, so that there is one *consequent point* in the middle of the core, this will behave like the combination of two independent cores of which each possesses the quantity of magnetism $\frac{m}{2}$, the polar interval $\frac{l}{2}$, and produces the quantity of heat $\frac{m^2}{4} \times \frac{l}{2}$. The total heat is therefore $\frac{m^2 l}{4}$; and this is the experimental law. Other cases are demonstrated in like manner.

The *differential thermomagnetic apparatus* above described is susceptible of an improvement by which the core is completely screened from the voltaic heat of the magnetizing coil. This purpose is accomplished by surrounding the *compensating* cylinder of iron with a wire like that of the magnetizing coil, with merely the difference that the current traverses each layer of wire in alternately opposite directions. In this way the compensating cylinder is submitted to the same perturbing influence as the magnetized cylinder, but no magnetism is developed in it; the manometer indicates only the effect of the heat, of magnetic origin, which is generated in the magnetized core.

I have thus constructed a very accurate apparatus, by means of which I can deduce the value in calories of the thermic effect ob-

served. I expect it will enable me to determine the *magnetic equivalent of heat*.—*Comptes Rendus de l'Acad. des Sciences*, March 23, 1874, vol. lxxviii. pp. 845–847.

EXPERIMENTAL RESEARCHES LEADING TO A DETERMINATION OF
THE TEMPERATURE OF THE SUN. BY FATHER A. SECCHI.

During the last summer I made some experiments for the purpose of determining the ratio between the solar radiation and that of the electric light, in order to solve, if possible, the question of the temperature of the sun. I chose this source of light as that which is the least remote in intensity from that of the sun, so as to lessen the divergence of opinion to which we have been conducted concerning the law of radiation, according as the theory of Newton or that of Dulong and Petit has been adopted.

To estimate the two radiations I have employed the same apparatus, the thermoheliometer described in my work *Le Soleil*. This instrument, notwithstanding the objections which have been made to it, appears to me especially suitable for determining, as in the present case, simple differences. If I_s and I_c denote the absolute intensities of the radiations of the sun and the carbon points, θ_s and θ_c the excesses of temperature of the black-bulb thermometer above the temperature of the vicinity in the cases of the solar and the electric radiation respectively, and α and δ the apparent diameters of the radiant surfaces viewed from the centre of the blackened thermometer, we have

$$\frac{\theta_s}{\theta_c} = \frac{I_s \tan^2 \delta}{I_c \tan^2 \alpha},$$

whence

$$I_s = I_c \frac{\theta_s \tan^2 \alpha}{\theta_c \tan^2 \delta}.$$

In practice it is very difficult to determine the radiating surface of the carbons: they are in general very brilliant at the point; but thence their incandescence diminishes rapidly, and, besides, the arc which separates them has a very different radiation. We have sought to determine the surface of the radiating part of the carbons by comparing their dimensions with those of glass tubes placed very near, and valuing the distance at which a fine platinum wire entered fusion without touching them. We have thus obtained a surface almost rectangular, equal to that of a circle of 1 centim. diameter. The radiation from the parts outside this limit was intercepted by diaphragms. The pile consisted of 50 Bunsen elements supplied with fresh acids (nitric acid of 40 degrees, sulphuric acid diluted with nine times its weight of water). The elements had a diameter of 12 centims., and a depth of 20 centims. The conductors, of copper, were short and very stout; the intensity of the current was such that the insulating disks of a Foucault apparatus were almost instantly melted, and an iron wire 1 millim. in diameter and 2·5

metres long could constantly be brought to a white heat. Doubtless these are very vague indications for estimating the temperature and the intensity of the current; but they suffice to give an idea of the conditions of the experiment.

The thermohelionometer being placed on a level with the carbons, and the blackened thermometer at a distance of 395 millims., there was found, at the end of half an hour, a difference of $3^{\circ}63$ between the temperature of the other thermometers and that of the black-bulb; this difference remained constant for an hour, the fluctuations being insignificant.

The temperature produced by solar radiation was determined about noon on several days in July, and with the same instrument. For the difference there was found $17^{\circ}16$, a quantity which agrees very well with that which was obtained several years since. To this value, however, must be applied the correction due to the absorption produced by our atmosphere; taking into account the height of the sun at the time of the observation, we are led to the value $17^{\circ}37$.

Substituting these values in the preceding formula and calculating the diameters α and δ from the dimensions and distances of the radiant areas, we get

$$I_s = I_c \times 36.468;$$

so that the solar radiation would be thirty-six and a half times that of the carbons. Nevertheless this valuation is below the truth; for we know that the correction made for the absorption by the atmosphere is too small. M. Soret found directly, on Mont Blanc, $21^{\circ}13$; probably the value at the upper limit of our atmosphere would be about 27° . These two values would give respectively—

$$21^{\circ}13 \dots\dots\dots I_s = I_c \times 44.36,$$

$$27^{\circ} \dots\dots\dots I_s = I_c \times 56.66.$$

These ratios differ considerably from those which have been given by other observers. In the fear that there might be some cause of enormous error in my electric light, I compared it with the light of a stearine taper; I found that it was equal to 1450 tapers of commerce; it possessed therefore quite the intensity ordinarily exhibited by a good pile. In another series of experiments, made after the pile had been worked for some time, I found $I_s = I_c \times 47.5$, a result not very far from that at which I arrived above with the temperature $21^{\circ}13$ obtained directly by M. Soret.

Adhering, then, to this temperature of $21^{\circ}13$, which is incontestable and certainly less than the reality, and supposing that the temperature of the radiating surface of the carbons is 3000° (a number which is not exaggerated, since the whole extent of the platinum submitted to the experiment was fused), and supposing the radiation proportional to the temperature, we obtain for the potential temperature of the sun 133780° . This value may even

be raised to 169980° by adopting the number of 27° as produced by the solar radiation.

I have already remarked, in my work on the Sun (p. 270), and M. Hirn has lately repeated it, that the temperature of the radiation may depend either solely on the superficial stratum of the sun, or on a considerable thickness of its substance, according as this is opaque or transparent. M. Hirn concludes that, if the transparency were nearly perfect, the temperature might well be only a few thousand degrees; but divers phenomena prove that, on the contrary, the transparency is very imperfect. In one of my preceding communications I reported the singular observation that the currents of the penumbra cross and pass one above another; in that case the upper currents completely conceal the lower; so that the mass of the photosphere has no sensible transparency. This observation of the crossing of the currents has lately been confirmed by Mr. Langley*. The defect of transparency can also be established by observation of the thick jets of the metallic protuberances, in which one branch does not permit the other to be seen through its thickness. Without admitting absolute opacity (for the very strong light of the photosphere may well prevent the lower strata from being distinguished), it is certain that the photosphere is not completely transparent; for otherwise the margin of the sun's disk would not appear sharply defined, but diffuse.

The temperature above indicated is therefore not inadmissible. It is doubtless very far from the number which would be given by the direct application of Newton's law; but it is also very far from that which would result from the law of Dulong and Petit. It seems to me that these comparative experiments almost completely eliminate the considerations drawn from the theoretic law, and that they give a lower limit of the temperature of the sun.

Besides, if the solar temperature reached only a few thousand degrees, its cooling would be sensible in a relatively brief interval of time; and this diminution of temperature would betray itself by a notable acceleration of the rotation of the sun. Doubtless the excess of velocity observed at the solar equator, which it has uselessly been attempted to account for by currents analogous to our trade-winds, is due to this cooling; for, though very slight, it is by no means *nil*. M. Faye's law, deduced from Carrington's, indeed results from the simple comparison of the diminution which the areas of the equatorial circles and the parallels must undergo by the cooling of the mass estimated in their respective planes of rotation. This progressive action must maintain constantly this difference of velocity; so that there is no need to have recourse, with M. Roche, to a remote crisis; for in that case the mass would soon have arrived at uniformity in consequence of friction.

The activity of the sun not being constant, it follows that neither are so its losses by radiation, and consequently that the apparent

* American Journal of Science, 3rd Series, vol. vii., February 1874.

rotation of the sun, or rather of its photosphere, is variable—which accords with the results obtained by Spörer.—*Comptes Rendus de l'Académie des Sciences*, March 16, 1874.

ON HYDROGENIZED PALLADIUM.

BY MM. L. TROOST AND P. HAUTEFEUILLE.

The remarkable property possessed by palladium, of absorbing as much as 982 times its volume of hydrogen gas, discovered by Graham, was at first presented by him as a phenomenon comparable to solution or condensation; and for it he invented the term *occlusion*.

Graham subsequently thought that palladium forms with hydrogen an alloy with "equal equivalents." This opinion is stated in the memoir in which he proves that the density of palladium charged with from 800 to 900 volumes of hydrogen is sensibly inferior to that of the pure metal, that the tenacity and electric conductivity are diminished as in the case of alloys in general, and the magnetism augmented as when palladium is combined with a very magnetic metal.

His conclusions have been generally accepted, although (as the author himself remarks) the maximum of 982 volumes of hydrogen fixed corresponds to only 0.772 of an equivalent of hydrogen for 1 equivalent of palladium ($H=1$; $Pa=106.5$).

In a recent work M. Favre, like Graham, thinks that "the hydrogen fixes itself on its equivalent of palladium," on the ground that, within the limits of his experiments*, the metal disengages sensibly equal quantities of heat in absorbing equal weights of hydrogen.

We shall prove that the phenomenon is more complex than has hitherto been supposed.

We will examine successively the two following points:—(1) Does the hydrogen form a true combination with the palladium, or is it merely dissolved in that metal? (2) If there is a combination, what is the formula of the compound produced?

The study of the tensions taken by the hydrogen disengaged at various temperatures by hydrogenized palladium will furnish us with the elements necessary to solve both these questions apart from any hypothesis. We know, in fact, that compounds formed directly, by the combination of a solid with a gaseous body, are partially decomposed by heat, the decomposition being measured by a tension invariable for each temperature and independent of the quantity of undecomposed product; it is the tension of dissociation of the chemical combination. On the contrary, substances

* M. Favre has not indicated the volumes of gas corresponding to his different determinations.

which have dissolved gases (such as water charged with carbonic acid), and those which have condensed it (such as platinum-black charged with hydrogen), emit gases which, for one and the same temperature, have tensions variable with the state of saturation of the material. It is by the study of the tensions that we have arrived at the recognition of the simultaneous production of a definite compound and of a solution of hydrogen gas.

The palladium, saturated with hydrogen at the negative pole of a voltameter, was introduced into a glass tube communicating at one extremity with a manometer, and at the other with a Sprengel pump, which permitted a vacuum to be produced at the commencement, and afterwards, in the course of the experiment, determined volumes of gas to be expelled*.

If the operation take place at about 100°, successively increasing quantities of gas being taken away, we obtain, with either cast or spongy palladium, the results inscribed in the following Table, which contains the observed pressures for different degrees of saturation.

Temperatures.	Cast palladium.		Spongy palladium.	
	Volumes of hydrogen fixed in the palladium.	Tensions.	Volumes of hydrogen fixed in the palladium.	Tensions.
		millim.		millim.
100°·2	809	1428	775	715
99·8	743	909	743	493
100	700	598	718	361
100	672	454	684	247
100·5	642	353	"	"
.....	...		...	...
99·8	610	238	608	227
100	595	225	590	225
100·1	402	230	300	224

The Table shows us :—(1) that as long as the volume of hydrogen fixed is more than 600 times the volume of the palladium, the pressure decreases very rapidly at each subtraction of hydrogen, which is the character of a solution ; (2) that the pressure becomes con-

* As palladium saturated with gas disengages hydrogen at the ordinary temperature, it is necessary, if we wish to know exactly the total volume of gas absorbed, to put the metal, on taking it out of the voltameter, into a small balloon full of boiling water and furnished with a discharge-tube. The temperature of the balloon is raised to the boiling-point of water, and the gas discharged is collected. After cooling, the metal can be handled in order to introduce it into the manometric apparatus without fear of losing any gas. This preliminary operation is indispensable when the forged metal is operated on, which, when withdrawn from the voltameter, grows rapidly hot in contact with the air, in consequence of the combustion of the hydrogen.

stant (which is the character of a combination) as soon as there are not more than 600 volumes of the gas—a quantity which corresponds to $\frac{1}{2}$ equivalent of hydrogen for 1 equivalent of palladium. From this moment the hydrogenized palladium behaves as a definite combination, susceptible of dissociation, and of which the tension does not depend on the temperature—after the manner of carbonate of lime, studied by M. Debray.

In order to exhibit more distinctly this invariability of pressure which is characteristic of combination, we will cite the results of an experiment made at the temperatures in the vicinity of 160° .

Temperatures.	Volumes of hydrogen gas fixed in the palladium.	Tensions of the definite compound.
		millim.
160 ^o	589	1475
161	574	1500
160.5	480	1480
161	290	1482
160	200*	1480

Series of observations made between 20° and 180° have taught us that within these limits of temperature the phenomenon retains the same characters; that is to say, the tension remains constant as soon as the quantity of fixed hydrogen is no more than 600 volumes, which corresponds to the formula Pa^2H . We have thus obtained the following numbers, which permit us to construct the curve of the dissociation tensions of the compound:—

* We have therefore been able, in the experiment cited, to take away two thirds of the gas without diminishing the tension. When the last traces of Pa^2H have been decomposed, the palladium remains saturated with dissolved hydrogen gas. We then reenter the conditions of a phenomenon of solution; and fresh subtractions of gas should bring a rapid decrease of the pressure: this is evidenced by the three following determinations:—

Temperatures.	Volumes of gas.	Tensions.
		millim.
161 ^o	62	731
160	38	435
161	29	391

Temperatures.	Dissociation tensions of Pa^2H .	Temperatures.	Dissociation tensions of Pa^2H .
	millim.		millim.
20°	10	100°	232
30	16	110	336
40	25	120	467
50	36	130	624
60	50	140	812
70	65	150	1104
80	106	160	1475
90	160	170	1840

Remarks.—The numbers in this Table were obtained with a palladium wire; this was preferred to a sheet for these determinations, because the boundary tension is more rapidly reached. Besides, a sheet leads, although more slowly, to the same results. The cast and the forged metal present no difference in respect of the tensions; and the behaviour is the same, whether the metal has been charged by the pile or by a prolonged stay in hydrogen.

The above numbers show that Pa^2H does not sensibly emit gas at the ordinary temperature. The hydrogen-tensions which limit the decomposition of the compound increase at first slowly; but from 140° very small additions to the temperature are attended by a rapid augmentation of the pressure. Between 130° and 140° the tension becomes equal to the pressure of the atmosphere.

This compound, then, cannot be prepared at a higher temperature than 130°, unless the hydrogen be compressed. The most convenient temperature for its production is that of boiling water, at which the dissociation tension is below one third of the atmospheric pressure.

In brief, our experiments prove that palladium forms with hydrogen a definite compound, of which the formula is Pa^2H . This combination, once formed, can dissolve hydrogen gas after the manner of platinum, and in quantity variable with its physical state. This property of Pa^2H explains the difference between the numerical results obtained by Graham according as he employed palladium wire or spongy palladium.

In another communication we will show that potassium and sodium form combinations with hydrogen which have the formulæ K^2H , Na^2H ($\text{K}=39$, $\text{Na}=23$). These, together with hydrogenized palladium, form a series parallel to that of which M. Wurtz discovered the first term in the combination of copper with hydrogen Cu^2H^2 ($\text{Cu}=63\cdot5$), and to which he gave the name of hydride of copper.—*Comptes Rendus de l'Acad. des Sciences*, vol. lxxviii. pp. 686–690.

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L. *On the new Contact Theory of the Galvanic Cell.*
By J. A. FLEMING, B.Sc., F.C.S.*

THE contest that has for so long been waged between the supporters of the two theories of the galvanic cell, the *contact* and the *chemical*, can hardly be said to have been brought even now to a decisive issue. For although the contact theory, as originally proposed by Volta, received a fatal blow when the law of conservation of energy became clearly understood, yet in its place a *new contact theory* has arisen, supported by novel and important experimental evidence, which has again been placed by recent writers on electrical science in formidable opposition to its old rival.

The old contact theory of Volta had its origin in an entire ignorance of the science of energy. It simply referred the current produced through the circuit of a pile to the effect of the metallic contacts, and it ignored the thermal and chemical changes which are also necessarily present; but it had to be finally abandoned when once it became clearly understood that the appearance of a current involved the disappearance of some other energy, actual or potential, as an invariable accompaniment. The new contact theory may be said to have had its source in the discovery of Sir W. Thomson, that there is undoubtedly a difference of potential produced when dissimilar metals are placed in contact—a fact not only abundantly proved by Thomson by direct experimental evidence, but, as he has

* Read before the Physical Society, March 21, 1874. Communicated by the Society.

pointed out, confirmed in a remarkable way by the phenomena of the Peltier effect, which, when interpreted by the dynamical theory of heat, furnish the most reliable measure of its amount. These facts, together with others presently to be referred to, have been made to furnish the key to a fresh explanation of the dynamics of the galvanic cell, which I have ventured to call the *new* contact theory, as opposed to the old or voltaic one.

It is not possible, however, to define in a few words the precise details of the new theory; they can only be arrived at by collecting together the statements as we find them laid down by their authors. The object of the present paper is to draw the attention of those interested in this question to the objections that may be raised against this new contact theory—objections based on facts, some old and some which perhaps may prove new, but all of which alike seem to throw fresh difficulties in the way of this theory, although capable of simple explanation by the old chemical hypothesis. It will be necessary then to review briefly the precise statements of this new contact theory, in order to show exactly what are the points against which objection may be taken. This will be best accomplished by collecting the statements of its principal supporters and arranging together their explanations of the phenomena which arise

(1) when dissimilar metals at the same temperature are placed in contact,

(2) when one insulated metal is placed in a liquid capable of acting chemically upon it,

(3) when two different metals are placed insulated and unconnected in one such liquid,

(4) when the two metals are joined across by a metallic arc or when two or more cells are joined up in series.

1. That the contact of metals is always attended with the production of a difference of potential between them was for a long time denied by ardent supporters of the chemical theory. De la Rive endeavoured to show that the effects observed might be attributed to oxidation; but his experiments are not conclusive; and to Sir W. Thomson belongs the credit of having established the fact by experiment, irrespective of his theoretical deductions from the facts of thermoelectricity. He thus describes his decisive experiment:—

“A metal bar insulated so as to be movable about an axis perpendicular to the plane of a metal ring, made up half of copper and half of zinc, the two halves being soldered together, turns *from* the zinc *towards* the copper when positively electrified, and *from* the copper *towards* the zinc when negatively electrified”*. The difference of potential he finds to be about .6

* Reprint of papers on Electrostatics and Magnetism, p. 316, § 400.

or $\cdot 7$ of that of a Daniell's cell when the metals are perfectly clean; but by oxidation of the copper it may be made equal to or even greater than that of a Daniell's cell. He has also shown that if zinc and copper cylinders be connected by a wire, the electrometer detects a difference between the potentials of the air in the interior, and, lastly, that if copper filings be allowed to fall from a copper funnel in contact with a vertical zinc cylinder, they convey a negative charge to a receiver placed below. Sir W. Thomson concludes that there is sufficient evidence to show that zinc and copper attract one another chemically at any distance if connected by a fine wire, and that, as Professor Tait remarks, "when *any* two bodies of different kinds are brought into contact, there is a certain amount of exhaustion of the potential energy of chemical affinity between them, and that the equivalent of this is, partly at least, developed in the new potential form of a separation of the so-called electric fluids, one of the bodies receiving a positive, the other a negative charge, the quantity depending on the nature and form of the bodies"*.

This is equivalent to saying that at the surface of contact there is perpetually a force tending to separate the two electricities in a direction perpendicular to that surface, while at all points ever so little within it there is no such force.

Professor Maxwell reiterates essentially the same facts. He gives Thomson's proof that the electromotive contact-force at a junction of two metals is represented by PJ , where P = the coefficient of the Peltier effect, or the heat absorbed at the junction due to the passage of a unit of current for a unit of time; and J is Joule's equivalent. He remarks that the electromotive force, as determined by this method experimentally, does not account for the whole electromotive force of a simple couple. This latter is in general far greater than that given by the Peltier effect for the same pair of metals. "Hence the greater part of Volta's force must be sought for, not at the junctions of the two metals, but at one or both of the surfaces which separate the metals from the air or other medium which forms the third element of the circuit"†.

Professor Jenkin, referring to these experiments of Thomson, adds that "In cases where no known chemical action occurs, as where zinc and copper touch each other, and yet difference of potential is produced, since this involves a redistribution of electricity, a small but definite consumption of energy must then occur; the source of this power cannot yet be said to be known"‡.

2. It seems to be universally admitted that when an insu-

* Thermodynamics, p. 62, § 107.

† Treatise on Electricity and Magnetism, vol. i. p. 302.

‡ Electricity and Magnetism, p. 55.

lated metal is placed in a liquid capable of acting chemically upon it, a difference of potential is produced between the metal and the liquid, a sudden rise in potential taking place in passing from the metal-surface to the liquid in contact with it, or that the metal becomes negatively and the liquid positively electrified, metals differing in the degree of electrification they can produce with any one electrolyte.

3. But if we ask what are the conditions when two different metals are so immersed, we find the most contradictory statements given. Sir W. Thomson expresses his opinion thus in 1862:—"For nearly two years I have felt quite sure that the proper explanation of voltaic action in the common voltaic arrangement is very near Volta's. I now think it quite certain that two metals dipped in one electrolytic liquid will (when polarization is done away with) reduce *two* dry pieces of the same metals when connected each to each by metallic arcs to the same potential"* , which seems equivalent to saying that there is no difference of potential produced other than that due to dissimilar contact. Thus also Professor Tait:—"By interposing between two metals which have been electrified by contact a compound liquid or electrolyte, these metals are at once reduced to the same potential—a result which could not have been obtained by connecting them by any metallic conductor. By the passage of the electricity a portion of the electrolyte is decomposed, and the potential energy thus developed is equal to that possessed by the electricity while separated in the metals"†.

Professor Jenkin advocates essentially the same views:—"When two dissimilar metals are plunged side by side into a liquid such as water or dilute sulphuric acid, they do not exhibit *any* sign of electrification; the three materials remain at one potential, or nearly so. If while the two dissimilar metals are in the liquid they are joined by metallic contact to terminal pieces of one and the same metal, these terminal pieces will be brought to the same difference of potentials as that which would be produced by direct contact between the dissimilar metals"‡. This amounts simply to saying that, as long as no wires are attached to the plates of a single cell, there is no difference of potential; but that when wires are joined on, the observed difference of potential is due to the contact of the *wire* with that metal plate to which it is dissimilar.

Again:—"When a single metal is placed in contact with an electrolyte, a definite difference of potentials is produced between them; zinc in water becomes negative, copper in water becomes

* Electrostatics, p. 317, § 400.

† Thermodynamics, p. 66, § 116.

‡ Electricity and Magnetism, p. 22.

negative, but less so than zinc. If, however, the two metals are plunged *together* into water, the copper, zinc, and water forming a galvanic cell, all remain at one potential, and no charge of electricity is observed on any part of the system." "If a piece of copper be now joined to the zinc, it (the copper) will become negative, and the other copper plate positive, the difference of potentials being that due to the direct contact between the zinc and piece of copper *only*, the water having the effect of simply conducting the charge from the zinc to the copper plate and maintaining them at one potential"*.

The foundation for these statements is found apparently in an experiment due to Sir W. Thomson. He finds that if half-disks of zinc and copper be arranged under a movable metallic needle maintained at a high positive potential, if they are connected by a wire or by contact, the needle moves in such a way as to show that the copper is negative and the zinc positive; while if they are separated by a slight interval and connected by a drop of water, *no* difference of potential is observed. Professor Jenkin also lays great stress on the fact that, whereas copper in contact with zinc becomes negative, in a single cell with wires attached it is the wire attached to the zinc that shows a negative potential. This he holds to be conclusive that the junction of the wire with the zinc plate is the real seat of the electrical separation; although he admits that there may be a slight difference due to the liquid, and that different liquids may augment or decrease this difference.

In another place he says:—"If the voltaic theory of the cell were absolutely correct, the electromotive force of the cell would depend wholly on the plates in the electrolyte, and not at all on the solution employed to connect them"†. But it has been found that the potential series of the metals is slightly changed by the solution employed to join the plates: in order to account for this fact it is necessary to treat the voltaic theory as incomplete. He adds, however, that the potential series of the metals for water, dilute acids, and ammoniac chloride do not differ so much as to invalidate the theory, although the series for alkaline sulphides is quite different and anomalous‡.

* Electricity and Magnetism, p. 44.

† Ibid. p. 215.

‡ It may be remarked in passing, that this identity of the potential series for different acids may perhaps arise from a different cause, and not be altogether such a proof of the contact theory as Professor Jenkin concludes it is. Andrews has shown that, when *one* metal combines chemically with *different* acids, the amount of heat liberated is the same, or nearly so. Hence, if the metals be arranged in the order of their heat-producing power when combined with the same acid, that order will remain the same for most other acids. But the order is quite different when the metals are combined with sulphur or oxygen. It is true that this order is not the

4. When the two plates in one electrolyte are joined by a wire, or when simple cells are joined up in series and the circuit closed by a wire, we find it stated that there is a constant separation of the electricities at the point of contact of different metals and a constant recombination, attended with decomposition, through the electrolyte. "Perhaps it is strictly accurate to say that the difference of potential is produced by the contact, and that the current which is maintained by it is produced by chemical action"*. And, lastly, that in a series of cells the electromotive force is due to the sum of the differences of potential produced by all the contacts.

The above quotations may be taken as affording the plainest notion of the new contact theory; and it will be seen that its fundamental propositions are briefly these:—

I. That two plates of different metals in one liquid are at the same potential when insulated and separated; *i. e.* there is *no* difference of potential due to chemical affinity.

II. In a cell series the gradual rise in potential, or the electromotive force, is due only to the dissimilar metallic contacts.

III. The chemical action in the battery is the result rather than the cause of the difference of potential, and is looked upon as an accompanying action rather than as the actual creator of the current, it having little or no share in the production of the difference of potential between the terminals.

These are, I venture to think, points not to be admitted as proved without further inquiry, and against which, as I shall hope to show, some grave if not insuperable objections may be urged, founded on other experimental evidence.

The first question to be settled is, then, whether in a series of cells the *whole* of the difference of potential between the terminals is due to the contacts, as above stated, or whether *any* portion is due to the tendency towards chemical combination existing between the metals and the electrolytes; and, as a consequence, whether in a single cell the plates are at the same potential or at different potentials, owing to the difference of chemical action upon them. Now I think this point will be sufficiently proved if we can establish by experiment, (i) that a battery of cells can be constructed without any dissimilar metallic contacts and with terminal plates of the same metal, and

electrochemical one; but various causes may interfere to disturb it. At any rate it is sufficient to show that this fact of the partial identity of the potential series for different acids cannot by any means be claimed as conclusive of the contact theory. Moreover, although the *order* may be the same for the different liquids, we do not know that the *coefficients* are the same for each metal in every dilute acid.

* Electricity and Magnetism, pp. 53–55.

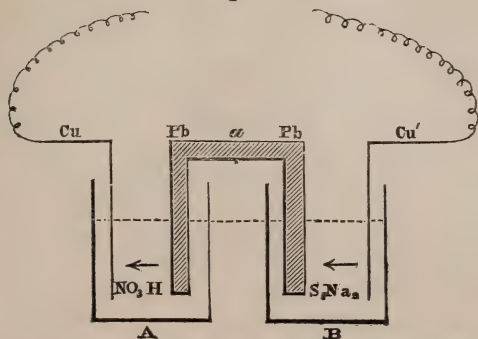
which shall yet exhibit difference of potential and continuous current; for if this is possible, it must follow that chemical affinity *alone* is capable of *creating* electromotive force as well as of maintaining a current, and that, in an ordinary cell-series, *some* part at least of the electromotive force is due to this cause, whilst the remainder is the result of the metallic contacts that may exist. Or (ii) if we can establish directly that the two plates in one cell are not at the same potential, as stated by more than one authority.

With regard to the first point, it will be remembered that an old experiment of Faraday's proved that a current can be maintained and decomposition effected by a single cell where there is no dissimilar contact. It is not easy to see how this experiment can be explained by any form of contact theory; indeed it appears unanswerable. But in order to leave no point unsettled by experiment, it seemed desirable to try and arrange a series of cells in which all dissimilar contact was absent, so that the difference of potential due to chemical action might be separated from that due to the contacts and rendered visible by the electro-scope.

It is obvious that we can make no attempt to do this unless we can in some way or other obtain a battery with terminals of the same metals; for otherwise the very junctions with the electro-scope introduce what we want to eliminate, viz. dissimilar metallic contact. But the following is a method by which this can be accomplished. If plates of lead and copper be placed in nitric acid the lead is positive to the copper, since it is most acted upon; but if lead and copper be placed in solutions of alkaline persulphides, then the copper is most readily acted upon and is positive to the lead; that is, the positions are reversed.

Now, if we place in a cell A dilute nitric acid and a copper

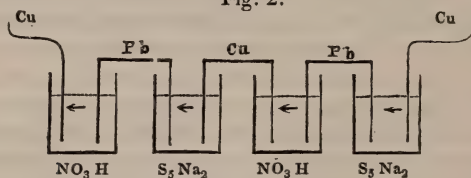
Fig. 1.



and a lead plate, we cannot join up another cell of the same sort

in series without introducing contact. But if, instead of using a cell containing acid, we place next A a cell, B, containing sodic pentasulphide, and bend over the lead plate of A to dip into the liquid in B, and place in B also a copper plate, we shall then have two cells joined up in series without dissimilar contact and with similar metals for terminals; and yet the action of the liquids on the metals is such that in A the lead is positive to the copper Cu, and in B the copper Cu' is positive to the lead. Hence there is a regular rise in potential in passing through the two cells; and on joining Cu Cu' by a copper wire a current flows through both cells in the same direction, and the general effect is to urge round a current in the direction shown by the arrows. It is obvious that we need not limit ourselves to two cells. By forming a pile of alternate cells filled with acid and alkaline persulphide, connected by bent copper and lead plates alternately (fig. 2), we shall be able to accumulate differ-

Fig. 2.



ence of potential to any extent; and if the number of acid and alkaline cells be equal, we shall always end with a plate similar to that with which we began. Such a battery will exhibit a difference of potential between its two terminals when the circuit is opened, and will give a current when it is closed. In it we have nothing but chemical action to rely upon both for creating electromotive force and for maintaining the current. We have no dissimilar contacts; and as the terminal plates are similar, we can effect the junctions with the electroscope without introducing an unbalanced dissimilar contact. I have constructed such a battery of 60 cells; and by the kindness of Professor Guthrie, to whom my thanks are due, I have been permitted to compare its potential with that of a Daniell's cell, by means of a quadrant electrometer belonging to his laboratory. By this means it is at once seen that the difference of potential increases proportionally to the number of cells, the electromotive force of four cells being about equal to that of one Daniell. Joined up with a galvanometer it indicates a current, which, however, rapidly falls off in strength, owing to the formation of an insoluble cupric sulphide upon the copper plates. Joined up in opposition to a single Daniell's cell, with a galvanometer included in the circuit, I find that it requires from four to five cells to balance the force of the Daniell at first immersion; but after leaving it to work on short

circuit for $2\frac{1}{2}$ hours its electromotive force had fallen off 50 per cent.; it then required about 8 cells to bring the needle to zero. This gives for the electromotive force of two cells at first about $\cdot 5$ of a volt; or the whole sixty cells are equal nearly to 15 Daniell's cells. It readily effects the decomposition of many electrolytes, and exhibits therefore every property of an ordinary cell-series. Above all, it will be noticed that since there is a regular rise in potential in passing from cell to cell, and as all parts of each plate must be at the same potential, that rise can only take place at the surfaces where the active metals are in contact with the electrolyte (that is, at the seat of the chemical action), and that therefore two metals in one electrolyte *cannot* be at exactly the same potential. But I find that more direct evidence still of this fact is to be found in an experiment of Faraday's, which seems to have escaped the notice of the contact theorists.

In his 'Experimental Researches' he gives the following fact. "I took a voltaic apparatus, consisting of a single pair of large plates, namely a cylinder of amalgamated zinc and a double cylinder of copper. These were put into a jar containing dilute sulphuric acid, and could at pleasure be placed in metallic communication by a copper wire connecting the two plates. Being thus arranged, there was no chemical action whilst the plates were not connected; on *making* the contact a spark was obtained. In this case it is evident that the first spark must have occurred before metallic contact was made, for it passed through an interval of air; and also that it must have tended to pass before the electrolytic action began, for the latter could not take place until the current passed, and the current could not pass before the spark appeared." "Hence," he says, "I think there is sufficient proof that the zinc and water were in a state of powerful *tension* previous to the actual contact"*. It is difficult to reconcile this with the experiment of the half disks and drop of water made by Sir W. Thomson. But, at any rate, a consideration of the whole of the facts would seem to point out that the only safe conclusion is, that in any series of cells of any sort the electromotive force is a complex effect, being due to the algebraical sum of all the differences of potential due to dissimilar contacts *plus* the algebraical sum of the differences of potential due to the chemical affinities of the metals and electrolytes *minus* any opposing force due to polarization &c.; and that so far from being the exclusive cause, the contacts can only be said strictly to have a share in producing the difference of potentials between the extremities of a battery†. And, lastly, we may with

* Experimental Researches in Electricity, Series viii. ¶ 956.

† Amounting in a Daniell's cell perhaps to 60 or 70 per cent. of the whole electromotive force.

advantage compare the statements of the contact theory with certain other well-ascertained facts. Such statements, for instance, as these:—"If we close the circuit by connecting the metals by a wire, we then have constant separation of electricities at the point of contact of different metals, and constant recombination attended with decomposition through the electrolyte"*. "The electricities separated at the metallic junctions recombine through the water," "whilst the current flows the water is decomposed"†,—which seem based on the assumption that the principal seat of the electrical actions is *not* to be looked for at the seat of the chemical actions. But, now, how does this fit in with those cases of electrochemical inversions noticed by De la Rive, where the direction of the current in a cell is *reversed* by simply diluting the electrolyte. Thus zinc is negative to tin in strong nitric acid, and mercury negative to lead; but in weak nitric acid the positions are reversed. Hence, if couples be formed of these metals in strong nitric acid, and the acid be gradually diluted, the current first ceases and then is reversed in direction.

Here, without altering the metallic junctions, we can at pleasure alter the direction of the current, and therefore also the direction of the fall in potential, since the current must flow from high to low potential. This seems conclusive that the chemical electromotive force must be even greater than the contact electromotive force. This reversal of the current, by changing the seat of the chemical activity, may be shown in another way, depending on the application of a very old principle. If plates of copper and clean iron be connected by copper wires with a galvanometer, and the iron rendered passive by immersion for a moment in strong nitric acid, then if these plates are plunged into dilute nitric acid the galvanometer indicates a strong current going through the cell from the copper to the iron. If they be removed for an instant and the iron plate touched, on again immersing the current is found to be reversed. Or we may again change the conditions, and notice that it is not sufficient to have merely two different metals and an electrolyte to form a cell. If plates of pure gold and platinum be placed in nitric acid, the most delicate galvanometer detects no current, and the same for many other pairs of metals and electrolytes.

Here we have contact of different metals producing its difference of potential; yet no current flows round "decomposing the electrolyte," as, according to the contact theory, it should do; but the instant we give play to chemical combination the ordinary results ensue. If the extremities of the copper wires from a galvanometer be attached to iron plates, and these plunged

* Tait, 'Thermodynamics,' § 116.

† Jenkin, 'Electricity and Magnetism,' p. 54.

into separate cups of dilute nitric acid, on making connexion between the two cups by a bent iron plate dipping into each no current is detected. On making one limb of the connecting plate passive and re-immersing, a strong current is visible; and we find that we have the direction of the current completely under command by making any of the four plates more or less acted on than the other three.

If these experiments are to have any importance attached to them, it can scarcely be doubted that they land us in conclusions similar to the others, namely:—that we must look for the principal source of the electrical disturbance at that place where the greatest chemical activity is being brought into play; and that whereas contact of metals is in itself productive of definite electrical separation, there is in the battery another cause assisting in the production of difference of electrical potential between the terminals, viz. the potential chemical combination between the metals and electrolytes existing when the circuit is open—the energy of the current produced when the circuit is closed being, of course, the equivalent of this potential energy which disappears.

LI. *The Boundary-Conditions of Reflection and Refraction for the Principal Section of Media in motion.* By Professor KETTLER, of Bonn*.

WHILE investigating the intensity of reflected and refracted light, I have arrived at equations which, as the most general, I believe include every possible special case, and therefore seem to deserve a peculiar interest.

Imagine two isotropic media (or even two anisotropic, under the limitation that the planes of symmetry of both coincide with the plane of incidence) divided by a partition, and both moving in space, *i. e.* in the still æther, with any velocity of translation, provided only it be small in comparison with the velocity of light.

I assume that the light incident on the dividing surface is linearly polarized; and accordingly I distinguish two cases—that its vibrations (1) are perpendicular to the incidence-plane, or (2) coincide with it.

For the first case two pure continuity-conditions are sufficient, namely the equations

$$\left. \begin{array}{l} \rho_E + \rho_R = \rho_D \\ c_E + c_R = c_D \end{array} \right\} x = 0,$$

* Translated from a separate impression, communicated by the Author, from the *Monatsbericht* of the Berlin Academy of Sciences, Jan. 8, 1874.

in which ρ denotes the vibration-excursions, c the velocities of vibration, and the suffixes specify whether they refer to the incident, reflected, or transmitted undulation. Both equations hold for the particles of the boundary-surface, consequently for $x=0$, when, namely, through it is placed a system of coordinates, moving with it, such that the x -axis coincides with the perpendicular, and, perhaps, the z -axis is perpendicular to the plane of incidence.

The first is identical with Cauchy's well-known equation of continuity; the second is a generalization of Fresnel and Neumann's, containing instead of the maximum velocities of oscillation their variable values.

If we write them thus,

$$\left. \begin{aligned} \rho_E + \rho_R &= \rho_D \\ \frac{d\rho_E}{dt} + \frac{d\rho_R}{dt} &= \frac{d\rho_D}{dt} \end{aligned} \right\} x=0,$$

and substitute in them the known cosine-form, A denoting the amplitude, T the time of an oscillation, taking into account the translation of the coordinate-system by which T_R and T_D are determined (in correspondence with Doppler's principle), they reduce to

$$A_E + A_R = A_D,$$

$$\frac{A_E}{T_E} + \frac{A_R}{T_R} = \frac{A_D}{T_D},$$

or, abbreviated,

$$\left. \begin{aligned} A_E + A_R &= A_D, \\ C_E + C_R &= C_D, \end{aligned} \right\} \dots \dots \dots (I.)$$

when the amplitudes A accompany the maximal velocities C of oscillation. The two equations are sufficient by themselves for the deduction of the intensity-formulæ, which, for isotropic media in a state of rest, accord with those in Appendix H of my *Astronomical Theory of Undulation**. Certainly, applied immediately to the same, the two would become identical.

For the second case we have the combination of the principle of the conservation of *vis viva* with the principle of the equivalence of the quantities of motion in the direction perpendicular to the *effective* dividing surface; for if by μ_E, μ_R, μ_D be understood the *reduced* masses of the æther, and by $\alpha_E, \alpha_R, \alpha_D$ the angles of incidence, reflection, and refraction reckoned from the

* *Astronomische Undulationstheorie, oder die Lehre von der Aberration des Lichtes.* Bonn, 1873.

normal of the effective surface, we can write :—

$$\left. \begin{aligned} C_E^2 \mu_E - C_R^2 \mu_R &= C_D^2 \mu_D, \\ C_E \sin \alpha_E \mu_E + C_R \sin \alpha_R \mu_R &= C_D \sin \alpha_D \mu_D^* \end{aligned} \right\} \quad \text{(II.)}$$

Therefore not merely will the *vis viva* of the reflected and the refracted wave result from that of the incident, but the algebraic sum of the just-noted components of the quantities of the motion in the incident and the reflected wave will also be equal to the corresponding components in the refracted wave.

I have here to elucidate the two new notions of the effective dividing plane and the reduced æther-masses.

In regard to the former, the particles of the incident wave do not all reach the moving partition in one and the same position. The effect of this is as if the partition were turned through a certain angle

$$\beta = \frac{g}{\omega} \sin \alpha \cos \psi,$$

where ψ denotes the angle between the perpendicular and the direction of the motion, and $\frac{g}{\omega}$ the ratio of the velocities of trans-

lation and light. The actual partition is thus, so to say, indifferent toward the incident wave; and the fictive rotating plane is at the same time also the effective one.

If, further, both media consisted of homogeneous æther of different densities, then the reduced masses would be equal to the actual ones. But now a ponderable transparent medium is an aggregate of particles of æther and particles of ponderable matter. Nearly the same constitution may be attributed to the æther in the interior of the medium as to the æther in cosmical space; and the material particles oscillating with it, which are certainly not hurried along entirely without resistance, but perform oscillations according to laws of their own (in the state of rest isochronously, and in general in smaller amplitudes than the æther particles), will act almost entirely as ballast, and usually contribute little to the development of force. Hence, if by m be understood the mass of æther, and by m' that of the body of the same volume, and if C , C' be the mean velocities of vibration corresponding to any wave of the individual æther- and body-particles, the velocity of propagation of this wave is given

* I prefer this form, first advanced by Cornu (*Ann. de Chim. et de Phys.* vol. xi. p. 283) for media at rest, to Fresnel and Neumann's continuity-equation, inasmuch as it brings out the distinction between velocity of oscillation and amplitude, and is applicable to crystals without bringing in other principles.

by the expression (*Astr. Und.* p. 195):—

$$\omega^2 = \frac{e}{m \left(1 + \frac{C'^2 m'}{C^2 m} \right)},$$

in which, evidently, instead of the oscillation-velocities C' , C , may be inserted the components taken along any direction, *e. g.* $C' \sin \alpha_D$, $C \sin \alpha_D$. The compound medium in question therefore behaves for every direction as a homogeneous æther would whose reduced mass was

$$\mu = m \left(1 + \frac{C'^2 m'}{C^2 m} \right) = m n'^2,$$

n' being the absolute index of refraction. If we write $\frac{C'}{C} = \frac{A' T}{A T'}$,

the ratio of amplitudes $\frac{A'}{A}$ appears as a constant characterizing the medium and remaining unaltered during the motion.

The further investigation is attended by a certain difficulty in ascertaining the three masses or volumes m_E, m_R, m_D set in motion under the influence of the translation. The solution of this problem, however, is facilitated by the fact that, referred to the moving isotropic medium, the continuity-conditions of Case I. permit the form of the equation of the *vires vivæ* belonging to them to be deduced; and this gives a hint for the behaviour of anisotropic media generally.

Lastly, with respect to the intensity-formulæ themselves which are thus to be obtained, we get, *e. g.* for the external reflection from moving isotropic media ($n'_E = 1$, $\omega_E = \omega_R = v$) and for the first principal case,

$$C_R = - \frac{\sin(\alpha - \alpha_D)}{\sin(\alpha + \alpha_D)} C_E;$$

and for the second,

$$C_R = - \frac{\tan(\alpha - \alpha_D)}{\tan(\alpha + \alpha_D)} C_E.$$

A moving mirror consisting of any isotropic medium behaves, therefore, as one of the same substance at rest, to which instead of the previous face a new one, turned the angle β , is given, and for which the former ratio of refraction n is in any way reduced to

$$n' = \frac{\sin \alpha}{\sin \alpha_D} = \frac{v}{\omega + g \kappa \cos(\alpha_D - \psi)},$$

in which ψ is referred to the component in the plane of incidence of the motion of translation, and κ signifies Fresnel's co-

efficient $\frac{n^2-1}{n^2}$. This transformation of the refraction-quotient is most simply effected by immersing the plate, without alteration of its internal constitution, in æther of the velocity of light—

$$\frac{v}{1 + \frac{g\kappa}{\omega} \cos(\alpha_D - \psi)}$$

And since also for the medium in motion the angles of incidence and reflection, reckoned from the new entrance-surface, are equal, the two systems would differ neither in intensity of reflection and refraction nor in any catoptric relation.

In the Astronomical Theory of Undulation (p. 180) it is further shown that in anisotropic as in isotropic media an equal motion always effects the same rotation of the wave-normal, equal to the angle

$$\delta\chi = \frac{g}{\omega} \frac{\sin(\psi - \chi)}{n^2};$$

and (p. 171) it is made clear that any combination of prisms of doubly refracting substances behaves in relation to the motion exactly like singly refracting ones. From this it is inferred that for anisotropic media also Fresnel's expression

$$n^i = \frac{v}{\omega + g\kappa \cos(\alpha_D - \psi)},$$

in which then

$$\kappa = \kappa_1 \sin^2 \chi + \kappa_2 \cos^2 \chi,$$

continues valid, provided only that ω denote the wave-velocity of the state of rest in the direction of the normal before, and not after its rotation. Hence even a crystal plate in motion will, in relation to the external reflection, be equivalent to one of the same substance at rest, if to the latter be given an entrance-surface rotated β from its initial position and it be immersed in æther of the before-mentioned velocity of light. The intensity of the light reflected in Case II. is obtained by means of Neumann and Seebeck's expression

$$C_R = - \frac{\sin \alpha \cos \alpha - \sin \alpha_D (\cos \alpha_D - \sin \alpha_D \tan \delta)}{\sin \alpha \cos \alpha + \sin \alpha_L (\cos \alpha_D - \sin \alpha_D \tan \delta)} C_E;$$

and therein δ signifies the angle between the ray and the wave-normal as a known function of the angle of orientation χ of the latter to the so-called axis of elasticity. To compare the action of the moving plate with that of a similar one at rest, on which in ordinary æther the light was incident at the same *apparent* angle e , it would be necessary, besides the reductions carried out

in the *Astr. Und.* (p. 223 *et seq.*) for isotropic media, to reduce δ to δ_0 , which value corresponds to the angle $\chi - \delta\chi$. The calculation relative to this, however (which, on account of the dissymmetry of the expressions that would have to be considered, would become tedious and complicated), we may the rather decline to carry out, as the result is absolutely void of practical consequence.

Not only, therefore, is there nothing to hinder the application of the boundary-equations (II.) even to the perfectly unsymmetrical occurrences at the hinder surface of a plate of glass or crystal in motion (conf. *op. cit.* pp. 230, 239), but the problem we have treated can itself be so far generalized that the hitherto presupposed coincidence of the plane of incidence with the plane of symmetry can be dispensed with.

LII. *On Warren's Method of finding Faults in Insulated Wires.*
By THOMAS T. P. BRUCE WARREN, *Electrician to Hooper's Telegraph Works, Limited*.*

THE following directions for performing this test are given by Mr. Latimer Clark (Clark and Sabine's Electrical Tables and Formulæ). The object of the present paper is to give the more recent methods of employing this test.

"The coil of wire is wound on two separate drums, both insulated, and an electrometer connected to each. A powerful battery is connected to one end of the conductor, the induction and leakage through the dielectric causing each of the electrometers to become deflected. Both drums are now discharged by touching them with the hand, and the electrometers fall to zero. The drum which has a defect on it, however, soon acquires its tension again, and its electrometer is deflected, the other remaining unaffected. More wire is then unwound till the fault appears on the other drum. The outside of the wire between the drums must be wiped very dry; the other parts should be moist."

This method of performing the test was soon abandoned from its being cumbersome and badly adapted for locating very minute faults, as by the use of two electrometers with different degrees of sensitiveness it was impossible, unless by carefully comparing them, to say on which drum the fault was. Although a galvanometer can be used for the test, an electrometer was preferred, from the fact that the deflected position of the needle or indicator is independent of the length under test, which, by the implied condition of the test, is variable. Obviously with a galvanometer the deflections alone could be but little depended upon.

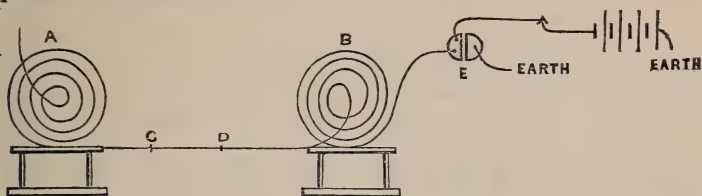
* Communicated by the Author.

The electrometers first used were a modified form of Coulomb's torsion instrument by Mr. Latimer Clark, which was replaced by a more sensitive arrangement of Peltier's electrometer which I had constructed. This in turn gave way to Sir William Thomson's quadrant electrometer, which under skilful manipulation is the most certain and convenient instrument, especially for demonstration, as a few yards of core will suffice for the experiment.

In practice an ordinary reflecting astatic galvanometer is generally used, in which case, instead of being guided by the deflections, we note the rush of current into the core when each drum alternately is connected to earth after a few moments' previous insulation.

I will first describe the method employed when an electrometer is used.

The length of wire to be operated on is immaterial, provided that the whole or a portion of it can be coiled on an insulated drum, and that between the parts coiled the surface of the core for a length of 6 or 8 inches can be cleaned and dried so as to prevent conduction.



In the first case (when the whole can be coiled on a drum), one half is coiled off on a second drum, and the two drums A and B afterwards carefully insulated. The surface of the core C D between the drums is well cleaned and dried. The conductor is attached to an electrometer, and the two drums are connected to earth by an attendant at each drum, when by connecting the battery to the electrometer and core the whole will become charged; the battery is then disconnected from the electrometer, and the earth-wires simultaneously taken off the drums. It is best to leave the battery on until the earth-wires are removed from the drums.

The insulation of the drums A and B and the electrometer E should be such that no loss can be perceived after a few minutes, when, if the earth-wire be applied first to one drum and then to the other, the fault will be found on that drum which causes the greatest fall in the electrometer. The wire is coiled from the faulty side to the other, and the test repeated as often as is required. A mile of core with a small fault in it can by a little

practice be put right in an hour or two, involving no more waste than the insulator, which can be held between the fingers, and without even cutting the conductor. The position of the fault can be reduced into a length represented by C D by cleaning and drying the surfaces on each side of it.

It is obvious if C D represent a joint, it may be tested by this method with great ease. In fact so delicate is this test when used for joints in this way, that if A and B each be 2 or 3 miles in length, and C D, a length of 10 inches, be slightly heated above the portions on A and B, the difference in insulation will be readily perceptible on the electrometer when this portion is connected to earth. The limits to the delicacy of the test are only reached when A and B are absolutely insulated.

A very interesting lecture-experiment may be formed from these circumstances.

Let A F represent a length of insulated wire (a series of insulated Leyden jars would answer as well). Clean the outer surface at B, C, D, and E; charge the core on an electrometer, connecting the portions A B, B C, C D, D E, and E F to earth. Remove



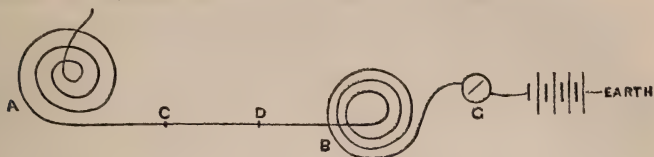
the earth-wires and finally the battery wire. We can if we wish remove the whole of the charge from any of these sections without affecting the charge in any of the other sections.

Suppose we wish to remove the charge from the section D E; we first connect the earth-wire to the surface between D and E, and touch A or the electrometer if still left on, when the needle will instantly fall to zero; we remove the earth-wire and afterwards test the other sections, when we shall find that their charges have been unaltered; and if the spaces have been well cleaned and dried, we shall find that, although the conductor is continuous, no charge will flow into D E; the charge in E F can be noted on the electrometer; and although a section D E between it and the electrometer be empty, it will not communicate a particle of its charge to it.

In the second case, where the bulk would prevent the whole from being insulated, we should continue to coil the core upon an insulated drum until the fault disappeared—that is, until it was coiled on the drum. This is a useful method when dealing with “served core” at a cable factory.

When a galvanometer is used, the corrections are as for ordinary insulation-tests. The galvanometer is short-circuited; and A and B, whilst connected to earth, are charged as before. The earth-wires are removed and the short-circuit key opened. The drums are after a few moments connected to earth alternately,

when the loss of charge is soon rendered visible on both sides ; but as the lengths on A and B may be very unequal, the rush



of current alone will not enable us to say on which side the fault may be ; but by carefully watching the electrification for an equal time on each side, no difficulty will be found in fixing upon the drum containing the fault.

Two or more faults existing together form no embarrassment to the test. All to be done is, if the faults are of unequal magnitude, to remove the more extensive first, or to keep one side right by removing the faults as they are coiled over to it. In any case the battery-power required will vary with the magnitude of the fault and the sensitiveness of the instrument.

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LIII. *A Contribution to the Theory of Resonators.*

By LORD RAYLEIGH, M.A., F.R.S.*

THE following paper is an extract from a work on Acoustics on which I have been for some time engaged ; on this account it is not quite self-contained, but will, I hope, be found sufficiently intelligible. Most of the materials have already been in my possession upwards of a year ; and the interest of the subject seemed to render further delay in publication inadvisable. Unfortunately the rapid completion of my book has been much interfered with by various causes of a private character.

The operation of a resonator when under the influence of a source of sound in tune with itself, has been the subject of much misapprehension, from which even Helmholtz does not appear to have been free. In a dictionary of science we read :—"The loudness of the sound produced by a sounding body is augmented by bringing the body into the neighbourhood of a column of air which is capable of vibrating in unison with itself"†. Now this statement, though true in certain cases, as, for example, when a tuning-fork is held over a resonator, requires, to say the least, serious limitation. The exceptions, if not more frequent, are,

* Communicated by the Author.

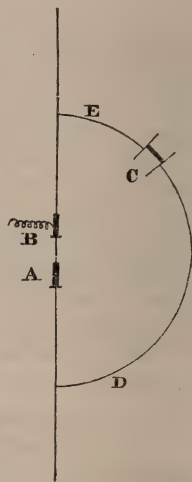
† Dictionary of Science. Rodwell. Art. Resonance.

from a theoretical point of view, more important than the cases in which the rule holds good. Indeed I should prefer to reverse the statement, and say that the neighbourhood of a resonator in unison with a sounding body *diminishes* the loudness thereof.

As the subject is rather a delicate one, we will begin by simplifying the conditions as much as possible. Let us suppose that the source of sound is a piston, A, imbedded in an infinite rigid plate. In the neighbourhood of A is another piston, B, backed by a spring, whose natural period is exactly the same as that of the vibration imposed upon A. ED is a rigid surface, enclosing A and B, and only allowing the communication of motion to the external atmosphere by means of a movable piston, C. The space enclosed by this surface is supposed to be occupied by gas devoid of inertia. It is easy to see that under these circumstances the piston C, though free to move, would yet remain at rest. For if the pressure within the vessel DE were in truth variable, the piston B would be acted upon by a force whose period was in exact agreement with that natural to itself, and its amplitude of vibration would increase without limit. The actual motion of B must be such as to leave the capacity of the vessel and the pressure constant; and then there is no force tending to move C, or rather to keep up the motion of C in the face of the dissipation which would be the necessary consequence of such motion. We may express this effect by saying that the condensations and rarefactions emitted by A are absorbed by B; and since if B were fixed, C would certainly move on account of the variation of pressure behind it, we see that the effect of the resonator is not to augment the sound, but, on the contrary, absolutely to stop it.

This conclusion does not depend on the rather artificial circumstances that we have here imagined. If the rigid walls represented by DE be removed, the same argument still shows that the pressure in the space surrounding AB must be invulnerable; and even if the inertia of the gas be restored, the general result will not be disturbed, provided that the distance AB is only a very small fraction of the wave-length, and that allowance is made for the inertia of the air in the neighbourhood of B in estimating the natural pitch of the resonator.

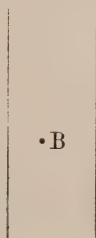
An instructive view of this question may also be obtained by means of the general principle of reciprocity established in



Chapter IV.* Let A be a simple source of sound at a distance from the mouth of the pipe. We know that at a point B (not too near the mouth) whose distance from the closed end is exactly $\frac{\lambda}{4}$, there is no variation of density. From this it follows,

•A

by the principle, that, if B were made a source of sound, there would be no variation of density at A—that is to say, that sound originating in B could not find its way out of the pipe. The restriction precluding too great a proximity to the mouth may be removed, if we suppose the source B to be uniformly diffused over any cross section of the pipe (distant $\frac{\lambda}{4}$ from the closed



end) instead of concentrated in one point. Here again the resonator may be said to absorb sound that would otherwise diffuse itself in surrounding space; or, if the non-emission of energy be thought incompatible with the existence of sound, we may say that the effect of the resonator is to dry up the source. For the present purpose it will be most convenient to use the expression "source of sound" in the sense of Chapter VIII., implying a given periodic production or abstraction of fluid, or something equivalent in its effect thereto, whether there be or be not emission of energy. The latter case will occur when there is on the whole no variation of pressure at the source itself.

We see then that, as far as external space is concerned, the neighbourhood of a resonator, far from augmenting the effect of a source, annuls it altogether, by absorbing the condensations and rarefactions into itself. The resonator acts, in fact, in the same way as would an equal and opposite source in the same position.

The principle here laid down, paradoxical as it will seem to many, is illustrated by the action of very simple apparatus, such as that employed by Quincke and others to stop sound of a particular pitch. Two varieties (figs. 1 and 2) are represented in

* See a paper by the author, "On some General Theorems relating to Vibrations," Proceedings of the Mathematical Society, June 1873.

the diagram, which, though somewhat different in appearance, are substantially identical.

Fig. 1.

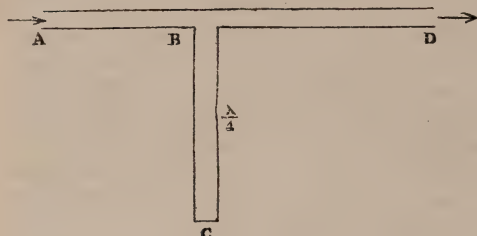
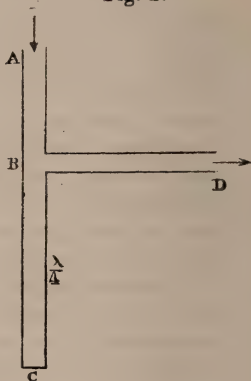
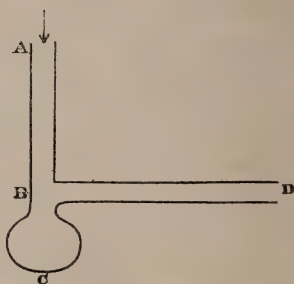


Fig. 2.



In both cases the sound enters, or rather is free to enter, at A; and at B finds itself at the mouth of a resonator B C, whose natural pitch agrees with its own. Under these circumstances the sound is absorbed, and there is no vibration propagated along B D. It is clear that the cylindrical tube may be replaced by any other resonator of the same pitch (fig. 3) without prejudice to the action of the apparatus.

Fig. 3.



The nature of the influence of a resonator on a simple source of sound in the immediate neighbourhood of its mouth will now be tolerably clear; the difficulty is rather to explain why the result is so different in the common experiment with the tuning-fork. Yet it is evident that a vibrating tuning-fork is something very different from a simple source of sound. The advancing face of either prong may be treated as a diffused source tending to produce a condensation; but this is always accompanied by an equal and opposite source due to the retreating face. Even if we confine ourselves to a single vibrating bar, the source cannot be approximately represented as any thing simpler than a double source, composed of two neighbouring equal and

opposite sources; the representation of the complete tuning-fork will require at least four simple sources. The fact is that the simple source of theory, or even a reasonable approximation thereto, is rarely met with in practice. When a solid body, such as a string, bar, or bell, vibrates, there is usually but little change of volume; and thus the algebraic sum of the sources is approximately zero, the effect depending mainly on the different situation of the positive and negative parts. It is true that the resonance-board of a musical instrument may be regarded as more nearly a simple source, in the sense that the principal component parts have the same sign; but the diffusion would generally be great enough to separate it considerably from the ideal simple source concentrated in one point.

In order to see how materially the action of a resonator depends upon the character of the source, we need only take the case of a double source, whose components are sufficiently far apart to be dealt with separately, but not so far that the interval amounts to a sensible fraction of the wave-length. In consequence of the last supposition, the joint effect at a moderate distance is but a very small part of what would be due to either component separately; the fluid emitted by the one is instantly sucked up by the other, and *vice versâ*. Now suppose that a resonator of the same pitch is presented to one of the component sources. Its effect, we have seen, will be to neutralize the source in question, and thus to leave the other free. The result is a considerable augmentation of loudness, which, however, to the eye of theory still appears as the absorption rather than as the production of sound.

In general, whenever a resonator is so situated that there would be at its mouth, supposed to be closed, a sensible variation of pressure due to external sources, the opening of the mouth will entail a great alteration in the mechanical conditions. There will be developed such a transfer of air backwards and forwards through the mouth as would, if it existed alone, produce a variation of pressure at the mouth equal and opposite to that above spoken of. Until this condition of things is attained (that is, until the total pressure at the mouth is constant) the vibration of the resonator must increase. Now the characteristic of a resonator is that vibrations escape from it slowly, that a large vibration within it corresponds to but a small variation of pressure at the mouth. Hence it follows that, under the supposed circumstances, there will be a large flow of air through the mouth, or, in other words, that the resonator will behave as a simple source of considerable power. If the system of external sources be of such a character as to produce by itself little effect, the operation of the resonator is attended by an augmentation of

loudness; otherwise it may happen that a part or the whole of the previously existing sound is extinguished.

The first case is that of the tuning-fork. In consequence of the comparative thinness of the prongs, the positive and negative sources nearly neutralize each other at a little distance, the air displaced by the advancing faces readily taking advantage of the room afforded by those retreating. Although the fluid moves nearly as though it were incompressible, there is in consequence of the inertia a certain variation of pressure, of which a resonator whose mouth is suitably placed can take advantage.

One of the best examples of this use of a resonator is afforded by a vibrating bar of glass or metal held at the nodes. A strip of plate glass about a foot long and an inch broad, of medium thickness (say $\frac{1}{8}$ inch), supported at about 3 inches from the ends by means of string twisted round it, answers the purpose very well. When struck with a hammer it gives but little sound except overtones; and even these may be nearly got rid of by choosing a hammer of suitable softness. This is a consequence of the small dimensions of the bar in comparison with the wave-length, which allows of the easy transfer of air from one side to the other. If now the mouth of a resonator of the right pitch* be held over one of the free ends, a sound of considerable force and purity is obtained by a well-managed blow. In this way may be constructed an improved harmonicon, with tones much graver than would be possible without resonators. In the ordinary instrument the wave-lengths are sufficiently short for the bar to communicate vibrations to the air independently.

The reinforcement of the sound of a bell by a resonator in a well-known lecture-experiment is an example of the same mode of action.

The other part of the theory (the neutralization of a simple source) is not so easily illustrated experimentally, for reasons already referred to. It usually requires more elaborate appliances to produce a simple than a compound source; and then there is a certain incongruity in the use of the word simple. The following experiment, however, deserves mention. Suppose that, in the manner just explained, we have obtained a pure sound by the combination of a resonator with a vibrating bar. In this arrangement the mouth of the resonator is the effective source; and according to theory we should expect that the sound would be again extinguished by the presentation to the first of a second resonator, held in such a position that the bar does not act upon

* To get the best effect the mouth of the resonator ought to be pretty close to the bar; and then the pitch is often decidedly lower than it would be in the open. The final adjustment may be made by varying the amount of obstruction.

it directly. This experiment succeeds perfectly: however strongly the bar be excited, there is but little sound heard. If about a second after the stroke the resonator be suddenly withdrawn, the swelling out of the sound is very striking, and proves conclusively that the presence of a resonator does not always augment the loudness of a sound. The second resonator may be held in such a position that it would by itself have little effect. The experiment may also be made with a tuning-fork held in the hand, or by some soft support.

The reader may perhaps be inclined to suppose, as I did at first, that the theory might be tested in the case of a simple tone produced in one room and heard in the next through a hole in the intervening wall. But a little consideration will show that the requirements of theory are not properly satisfied by this arrangement, in fact that the intensity of the source (at the hole) is itself largely dependent on the presence or absence of the resonator. Thus, if we suppose that the hole is small and the original source distant, the pressure in the neighbourhood of the hole on the first side of the wall will be nearly the same as if the hole were stopped; so that if the inertia of the air near the hole can be neglected, we have on the second side a given pressure, and not, as the theory assumes, a given flow. With a tuning-fork mounted on a resonance-box and held rather close to the hole, I have, however, succeeded in proving that the sound coming through the wall may sometimes be diminished by the neighbourhood of a resonator.

In order to obtain more exact ideas of the operation of a resonator, let us take one whose mouth is furnished with an infinite flange, and which vibrates in response to a source of any kind at a distance. The whole potential outside the resonator may be divided into two parts—one of which is the same as if the mouth of the resonator were closed, and the other the same as if there were no external source, but the motion at the mouth were maintained according to the same law as actually obtains. Over the area of the mouth, which is supposed small, the potential of the former part will be nearly constant, and may be denoted by ψ_0 . For the second part, ϕ_0 , we have already had occasion to form the expression

$$\begin{aligned}\phi_0 &= \frac{-1}{2\pi} \iint \frac{d\phi}{dx_0} \frac{\epsilon^{-i\kappa r}}{r} d\sigma = \frac{-1}{2\pi} \left\{ \iint \frac{d\phi}{dx_0} \frac{d\sigma}{r} - i\kappa \iint \frac{d\phi}{dx_0} d\sigma \right\} \\ &= \frac{-1}{2\pi} \iint \frac{d\phi}{dx_0} \frac{d\sigma}{r} + \frac{i\kappa}{2\pi} \dot{X},\end{aligned}$$

if $\dot{X}$ represent the total current. Of these two terms the first depends upon the inertia of air outside the mouth, whose effect is equivalent to an addition to the mass of the vibrating parts

of the resonator. We here suppose that the natural pitch of the resonator, agreeing with that of the external source, is estimated after allowance for this correction, and may then leave this term out of account. Otherwise, of course, the condition of synchronism is not properly satisfied. In order that the force tending to increase the vibration without limit may vanish, we must have

$$\phi_0 + \psi_0 = 0,$$

so that

$$\dot{X} = -i\lambda\psi_0.$$

When the pitch is given, the total current through the mouth of a resonator in synchronism with the source is independent of the form of the resonator, provided that the mouth is furnished with the flange. In the absence of a flange, the relation between $\dot{X}$ and ψ_0 would be sensibly altered. The effect of the resonator in external space depends only on $\dot{X}$, and is therefore always the same, but inside the reservoir will be inversely proportional to the capacity S . Hence, in order to get a great internal effect, the reservoir should be made small, and the pitch kept down to the required point by elongating the neck.

It is proper to state that the preceding theory is at variance with that given by Helmholtz*, who treats the potential ψ_0 as vanishingly small in comparison with that due to the resonator ϕ_0 . Helmholtz is thus led to the conclusion that a simple source would be intensified by the neighbourhood of a resonator. If it be thought strange that the very powerful motion within the resonator should produce no more effect than the external source, it must be remembered the small tendency to escape is the very reason of the accumulation.

LIV. *On Telegraphic Signalling with Condensers.*

By OLIVER HEAVISIDE, *Esq.*†

GIVEN an insulated conductor called the *line* connecting two places, there may be said to be in present use two distinct methods by which signals made at one end of the line are observed at the other. The first, which is that in most general use, is to connect the line with one end of the coils of an instrument affected by electric currents, and the other end of the coils with earth. The battery at the sending-end being also placed between the line and the earth, a *circuit* is established, through which a current will flow so long as the battery and instrument remain undisturbed. This current will in a short time after the first mo-

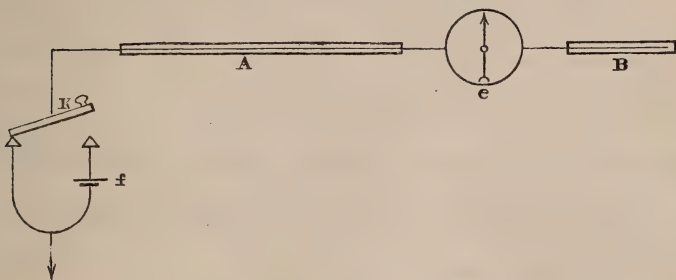
* *Tonempfindungen*, Beilage ix.

† Communicated by the Author.

ment of contact with the battery become approximately constant at any one part of the line—and, if there be no leakage, will attain the same strength at every part of the circuit, including the battery and receiving instrument. The second method, first introduced by Mr. Varley, and now in pretty general use on submarine lines, is somewhat different. The end of the coils of the receiving instrument, which in the first method is connected with the earth, is now joined to one armature or inductive surface of a so-called condenser, properly speaking an electrical accumulator, the other armature of which is to earth; or, which comes to the same thing, the condenser is placed between the line and the receiving instrument. As there is now no longer a complete conductive circuit, no permanent current can flow through the receiving instrument, or indeed through any part of the line, if the insulation be perfect.

Imagine the condenser to be a continuation of the cable, in fact a length of cable having the same capacity as the condenser, insulated at its further extremity, and the receiving instrument connecting the main cable with its imaginary continuation, as shown in fig. 1; where f is the battery at the sending-end of the line, one pole of which is to earth, K a key for making contact between its other pole and the cable A , and e the receiving instrument placed between A and the continuation B .

Fig. 1.



Then when contact is made at K it is evident that only so much electricity will pass through e as will charge B up to the potential of the further end of A . The current through e will therefore be transient, rising to a maximum and then dying away. This method of representation would be perfect if we could neglect the resistance of the conductor inside B ; as, however, in practice the capacity of the condenser is only a fraction of that of the line, there will be little difference due to this cause. And if the capacity of B be very small, we may consider the flow

of current through e to be strictly dependent on the rise or fall of potential of the end of A.

To find an expression for the potential and the current at any point of a cable insulated at one end, at any time after contact is made with a battery at the other end, the only way, as far as I am aware, is to follow the method given by Sir William Thomson in 1855 (Proc. Roy. Soc.), making the necessary alterations to suit the changed conditions of the problem. It is to express the actual potential at any time as the difference of two functions, one being the known final distribution of potential, and the other the departure from the final potential, the latter being expressed by an infinite convergent series every term of which is of the form

$$\sin x \cdot e^{-t}.$$

Let l be the length of the line,

k the electrical resistance of the conductor per unit of length,

c its electrostatic capacity per unit of length,

k_1 the resistance of the dielectric per unit of length to conduction in a radial direction,

V the electromotive force of the battery, the resistance of which is neglected,

v the potential, and

C the current at any point x of the conductor, measured from the battery-end, at the time t from the moment of making contact.

The differential equation of conduction in a telegraphic line is

$$ck \frac{dv}{dt} = \frac{d^2v}{dx^2} - h^2v, \quad . \quad . \quad . \quad . \quad (1)$$

where $h = \sqrt{\frac{k}{k_1}}$; and we must find a solution of this to satisfy the following conditions, which are given by the circumstances of the case.

1. $v = V$ when $x=0$.

2. $\frac{dv}{dx} = 0$ when $x=l$.

3. $v = 0$ when $t=0$, except when $x=0$.

4. $v = f(x)$ when $t = \infty$.

To find the function $f(x)$ expressing the permanent distribution of potential after an infinite time, make $\frac{dv}{dt} = 0$ in (1), and integrate the resulting equation

$$\frac{d^2v}{dx^2} = h^2v,$$

subject to the first and second conditions. We thus obtain

$$f(x) = V \cdot \frac{\epsilon^{h(l-x)} + \epsilon^{-h(l-x)}}{\epsilon^{hl} + \epsilon^{-hl}} \quad . \quad . \quad . \quad (2)$$

for the final distribution.

In expanding (2) in a series of sines we must remember that $\frac{dv}{dx} = 0$ when $x = l$, and accordingly use the expansion

$$f(x) = \frac{2}{l} \sum_1^{\infty} \sin \frac{(2i-1)\pi x}{2l} \int_0^l f(x') \sin \frac{(2i-1)\pi x'}{2l} dx',$$

which gives

$$\begin{aligned} f(x) &= V \frac{\epsilon^{h(l-x)} + \epsilon^{-h(l-x)}}{\epsilon^{hl} + \epsilon^{-hl}} \\ &= 4\pi V \sum_1^{\infty} \frac{2i-1}{(2i-1)^2\pi^2 + 4l^2h^2} \cdot \sin \frac{(2i-1)\pi x}{2l}; \end{aligned}$$

consequently the required solution is

$$\begin{aligned} v &= V \frac{\epsilon^{h(l-x)} + \epsilon^{-h(l-x)}}{\epsilon^{hl} + \epsilon^{-hl}} \\ &- 4\pi V \cdot \epsilon^{-\frac{h^2t}{ck}} \sum_1^{\infty} \frac{2i-1}{(2i-1)^2\pi^2 + 4l^2h^2} \cdot \sin \frac{(2i-1)\pi x}{2l} \cdot \epsilon^{-\frac{(2i-1)^2\pi^2t}{4ckl^2}}. \quad (3) \end{aligned}$$

When the insulation is perfect and $h=0$, this becomes

$$v = V - \frac{4V}{\pi} \sum_1^{\infty} \frac{1}{2i-1} \cdot \sin \frac{(2i-1)\pi x}{2l} \cdot \epsilon^{-\frac{(2i-1)^2\pi^2t}{4ckl^2}}. \quad . \quad . \quad (4)$$

As the current equals $-\frac{1}{k} \frac{dv}{dx}$, we have, by differentiating (3) and (4),

$$\begin{aligned} C &= \frac{Vh}{k} \cdot \frac{\epsilon^{h(l-x)} - \epsilon^{-h(l-x)}}{\epsilon^{hl} + \epsilon^{-hl}} \\ &+ \frac{2V}{kl} \cdot \epsilon^{-\frac{h^2t}{ck}} \sum_1^{\infty} \frac{(2i-1)^2\pi^2}{(2i-1)^2\pi^2 + 4l^2h^2} \cdot \cos \frac{(2i-1)\pi x}{2l} \cdot \epsilon^{-\frac{(2i-1)^2\pi^2t}{4ckl^2}}; \quad (5) \end{aligned}$$

and when $h=0$,

$$C = \frac{2V}{kl} \sum_1^{\infty} \cos \frac{(2i-1)\pi x}{2l} \cdot \epsilon^{-\frac{(2i-1)^2\pi^2t}{4ckl^2}}. \quad . \quad . \quad (6)$$

We can employ (5) and (6) to determine the flow through the receiving instrument, by giving x a value something less than l ; but it is preferable to use the series for $\frac{dv}{dt}$ obtained from (3) and (4) by differentiation.

$$\left. \begin{aligned} \frac{dv}{dt} &= \frac{4V}{\pi} \cdot \frac{h^2}{ck} \cdot \epsilon^{-\frac{h^2 t}{ck}} \sum_1^\infty \frac{1}{1 + \frac{4l^2 h^2}{(2i-1)^2 \pi^2}} \cdot \frac{1}{2i-1} \cdot \sin \frac{(2i-1)\pi x}{2l} \\ &\quad \cdot \epsilon^{-\frac{(2i-1)^2 \pi^2 t}{4ckl^2}} \\ &+ \frac{V\pi}{ckl^2} \cdot \epsilon^{-\frac{h^2 t}{ck}} \sum_1^\infty \frac{1}{1 + \frac{4l^2 h^2}{(2i-1)^2 \pi^2}} \cdot (2i-1) \cdot \sin \frac{(2i-1)\pi x}{2l} \\ &\quad \cdot \epsilon^{-\frac{(2i-1)^2 \pi^2 t}{4ckl^2}} \end{aligned} \right\} (7)$$

This becomes, when $h=0$,

$$\frac{dv}{dt} = \frac{V\pi}{ckl^2} \cdot \sum_1^\infty (2i-1) \cdot \sin \frac{(2i-1)\pi x}{2l} \cdot \epsilon^{-\frac{(2i-1)^2 \pi^2 t}{4ckl^2}} \quad (8)$$

A unit of time of a very convenient magnitude for practical calculations is

$$a = \frac{ckl^2}{10\pi^2} \log_e 10.$$

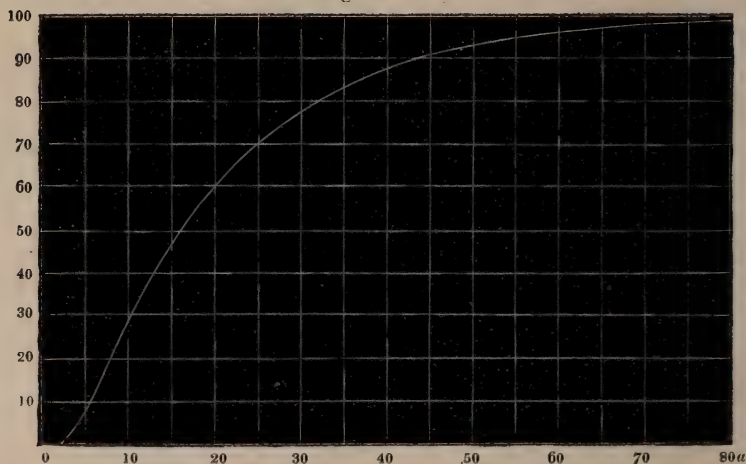
Employing this unit, we have the following series for v and $\frac{dv}{dt}$ when $x=l$ and $h=0$:—

$$v = V - \frac{4V}{\pi} (10^{-\frac{t}{40a}} - \frac{1}{3} \cdot 10^{-\frac{9t}{40a}} + \frac{1}{5} \cdot 10^{-\frac{25t}{40a}} - \&c.) \quad (9)$$

$$\frac{dv}{dt} = \frac{V\pi}{ckl^2} (10^{-\frac{t}{40a}} - 3 \cdot 10^{-\frac{9t}{40a}} + 5 \cdot 10^{-\frac{25t}{40a}} - \&c.) \quad (10)$$

The “arrival-curve” for v , calculated from equation (9), is shown in fig. (2).

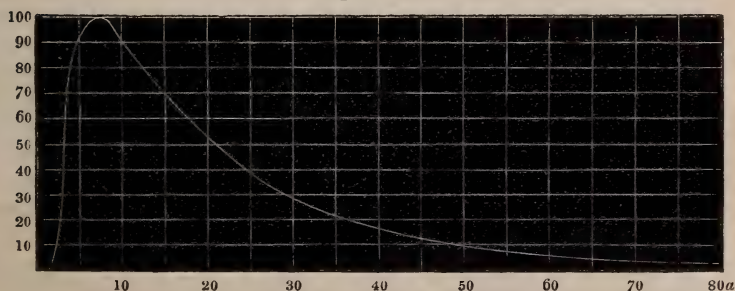
Fig. 2.



By comparison with the arrival-curve for the *current* at the remote end when to earth, we see that, broadly speaking, it takes about four times as long for the potential to nearly attain its maximum when the end is insulated as it takes for the current to nearly attain its maximum when the end is to earth. Thus, when the end is to earth, the current reaches 98 per cent. of its maximum strength in $20a$; and when the end is insulated, the potential reaches 98 per cent. of its maximum in $80a$. This relation does not hold good throughout the whole extent of the curves; but there is a general similarity. We may conclude that signalling by means of an electrometer connected with the insulated end of a cable would be much slower than the ordinary plan of a galvanometer or recording instrument worked by the current.

Fig. 3 represents $\frac{dv}{dt}$ from $t=0$ to $t=80a$ calculated from equation (10), and is closely the same as the arrival-curve for the current in condenser signalling. It will be seen that $\frac{dv}{dt}$ reaches its maximum in $7a$.

Fig. 3.



The strength of the current will of course depend on the capacity of the condenser, and will be proportional thereto so long as it is small compared with the capacity of the line. As, however, an increase in the capacity is equivalent to lengthening the line, the maximum strength of the current will not be so soon reached with the larger capacity: although the signals will be stronger, they will be more retarded. Hence the best capacity to be used on any line, which should theoretically be as small as possible, must be determined by the delicacy of the instrument and the battery-power employed. When the capacity of the condenser is one seventeenth part of that of the line, the maximum strength of a signal is about one tenth of the per-

manent current which would flow were the end of the line put to earth.

The effect of a condenser on the signals varies according to the description of instruments used for observing the signals, on the system of currents forming the signals, and on the value of the unit of time a for the particular line under consideration. Thus on an overland line worked on the Morse system with single currents, where the signalling consists in alternately connecting the sending-end of the line with one pole of a battery and to earth, it would be impossible to make *dashes*, because on land lines the length of a contact is always so great compared with the unit a , even in rapid signalling, that the current at the receiving-end would have come and gone long before the contact at the sending-end was finished. On cables of any considerable length it would be different. There would then be only a shortening of the dashes, its extent depending on the length of the contact; and it would consequently lead to an increase in the speed of working. There would similarly be very little direct advantage, save immunity from earth-currents, in using on land lines the condenser with polarized relays and reversing-keys, although there would be no shortening of the marks, as the armature of a polarized instrument will, if properly adjusted, remain in the position in which it is placed by a transient current. On cables worked with the same instruments, a considerable increase in the speed of working results.

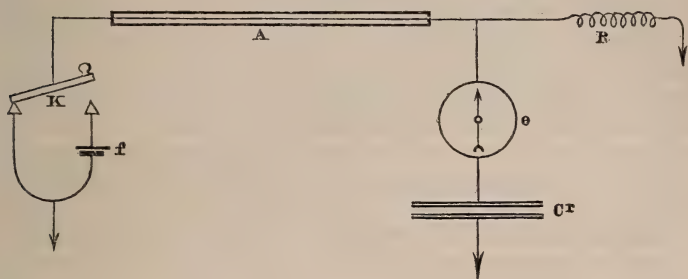
But the condenser is peculiarly applicable to those systems of signalling where the currents sent are of equal duration and alternately positive and negative; for example, Sir C. Wheatstone's automatic system. A succession of reversals, each contact of the length $4a$, produces through the receiving instrument reversals alternately 50 per cent. *plus* and 50 per cent. *minus*, being a whole range of 100 per cent. Without the condenser the whole amplitude of variation of the current is only 24.42 per cent. On a certain circuit worked by the automatic system, on which the value of a was about 0.0175 second, the speed of working with condenser was 75 per cent. greater than without.

With Sir W. Thomson's mirror and recording instruments there does not at first sight appear any reason why the speed of working should be much raised, as they indicate in the one case and record in the other every change in the strength of the current. Yet the condenser is of great advantage here, as it keeps the spot of light and siphon within very narrow ranges, never departing much from the zero line, and naturally the signals are much more distinct.

It will be found on examination that $\frac{dv}{dt}$, when the end of the

line is to earth, reaches its maximum in $3a$, as against $7a$ when the end of the line is insulated. Thus it would appear that a considerable increase in the speed of signalling should result from connecting the line to earth through a resistance, as shown in fig. 4, where f is the battery, A the cable, e the receiving in-

Fig. 4.



strument, C^r the condenser, and R the resistance introduced between the end of the line and earth. This resistance is necessary in order to raise the potential of the end of the cable, and give signals of a workable strength. That this plan does increase the speed, I have verified on the circuit before mentioned, using a polarized instrument and double current key. The increase in speed was about 20 per cent., compared with the speed obtained by making R infinite. The signals were, of course, weaker in the former than in the latter case.

The effects of defective insulation may be traced by giving h different small values in equation (7). In the first place the signals are weakened; and next, the effect of loss is to decrease the time required for the signal to arrive at its maximum strength. Thus when $h = \frac{\pi}{l}$, the maximum is reached in $4a$, as against $7a$ when $h=0$. This is an extreme case, and would nearly correspond to the French Atlantic cable if its insulation-resistance were 3 megohms per knot, instead of more probably 300. Whether loss does or does not increase the speed of working depends on a great many circumstances. It is an undoubted fact that, under some systems of signalling, a cable with a very bad fault in it has worked quicker than when it was perfect; and it is also a demonstrable fact that under other systems the effect of loss is greatly to diminish the speed. In a particular case which I have examined theoretically and practically, the fact that each signal should be more quickly made through a faulty than a perfectly insulated cable is quite consistent with the fact that the speed of working is reduced.

Another method of receiving signals has been tried, though not adopted anywhere. The current is passed through the primary wire of an induction-coil, in the secondary circuit of which is the receiving instrument. The signal here depends on $\frac{dC}{dt}$, the rate of increase of the current; and the arrival-curve has its maximum at about $3.5a$, while with condenser pure and simple it is $7a$. It does not, therefore, appear evident why, as Mr. Varley states, he found the condenser more satisfactory. Mr. G. K. Winter reinvented this system, and reports very favourably on its effect (British Association, Brighton, 1872).

LV. *On the Physical Cause of Ocean-currents.*

To the Editors of the Philosophical Magazine and Journal.

GENTLEMEN,

IN a lecture at the Royal Institution, and also in the 'Athenæum,' 'Nature,' Philosophical Magazine, and other quarters, Dr. Carpenter has been advancing a somewhat plausible-looking objection to my views in reference to under-currents. As this objection bears upon a point which in my last communication I omitted to consider, perhaps you will permit me, through your columns, briefly to refer to it. The objection in question, as stated in Dr. Carpenter's own words, is as follows:—

"According to Mr. Croll's doctrine, the whole of that vast mass of water in the North Atlantic, averaging, say, 1500 fathoms in thickness and 3600 miles in breadth, the temperature of which (from 40° downwards), as ascertained by the 'Challenger' soundings, clearly shows it to be mainly derived from a polar source, is nothing else than *the reflux of the Gulf-stream*. Now, even if we suppose that the whole of this stream, as it passes Sandy Hook, were to go on into the closed Arctic basin, it would only force out an equivalent body of water. And as, on comparing the sectional areas of the two, I find that of the Gulf-stream to be about one 900th that of the North Atlantic underflow; and as it is admitted that a large part of the Gulf-stream returns into the Mid-Atlantic circulation, only a branch of it going on to the north-east, the extreme improbability (may I not say impossibility?) that so vast a mass of water can be put in motion by what is by comparison a mere rivulet (the north-east motion of which, as a distinct current, has not been traced eastward of 30° W. long.) seems still more obvious."

The objection seems to me to be based upon a series of misapprehensions:—(1) that the mass of cold water 1500 fathoms deep and 3600 miles in breadth is in a state of motion towards

the equator; (2) that it cannot be the reflux of the Gulf-stream, because its sectional area is 900 times as great as that of the Gulf-stream; (3) that the immense mass of water is, according to my views, set in motion by the Gulf-stream.

I shall consider these in their order. (1) That this immense mass of cold water came originally from the polar regions I, of course, admit, but that the whole is in a state of motion I certainly do not admit. There is no warrant whatever for any such assumption. According to Dr. Carpenter himself, the heating-power of the sun does not extend to any great depth below the surface; consequently there is nothing whatever to heat this mass but the heat coming through the earth's crust. But the amount of heat derived from this source is so trifling, that an under-current from the Arctic regions far less in volume than that of the Gulf-stream would be quite sufficient to keep the mass at an ice-cold temperature. Taking the area of the North Atlantic between the equator and the tropic of Cancer, including also the Caribbean Sea and the Gulf of Mexico, to be 7,700,000 square miles, and the rate at which internal heat passes through the earth's surface to be that assigned by Sir William Thomson, we find that the total quantity of heat derived from the earth's crust by the above area is equal to about 88×10^{15} foot-pounds per day. But this amount is equal to only $\frac{1}{894}$ that conveyed by the Gulf-stream, on the supposition that each pound of water carries 19,300 foot-pounds of heat. Consequently an under-current from the polar regions of not more than $\frac{1}{35}$ the volume of the Gulf-stream would suffice to keep the entire mass of water of that area within 1° of what it would be were there no heat derived from the crust of the earth; that is to say, were the water conveyed by the under-current at 32° , internal heat would not maintain the mass of the ocean in the above area at more than 33° . The entire area of the North Atlantic from the equator to the arctic circle is somewhere about 16,000,000 square miles. An under-current of less than $\frac{1}{17}$ that of the Gulf-stream coming from the Arctic regions would therefore suffice to keep the entire North Atlantic basin filled with ice-cold water. In short, whatever theory we adopt regarding oceanic circulation, it follows equally as a necessary consequence that the entire mass of the ocean below the stratum heated by the sun's rays must consist of cold water. For if cold water be continually coming from the polar regions either in the form of under-currents, or in the form of a general under-flow as Dr. Carpenter supposes, the entire under portion of the ocean must ultimately become occupied by cold water; for there is no source from which this influx of water can derive heat, save from the earth's crust. But the amount thus derived is so trifling as to produce no sensible

effect. For example, a polar under-current one half the size of the Gulf-stream would be sufficient to keep the entire water of the globe (below the stratum heated by the sun's rays) at an ice-cold temperature. Internal heat would not be sufficient under such circumstances to maintain the mass 1° Fahr. above the temperature it possessed when it left the polar regions.

(2) But suppose that this immense mass of cold water occupying the great depths of the ocean were, as Dr. Carpenter assumes it to be, in a state of constant motion towards the equator, and that its sectional area were 900 times that of the Gulf-stream, it would not therefore follow that the quantity of water passing through this large sectional area must be greater than that flowing through a sectional area of the Gulf-stream; for the quantity of water flowing through this large sectional area depends entirely on the rate of motion.

(3) I am wholly unable to understand how it could be supposed that this under-flow, according to my view, is set in motion by the Gulf-stream, seeing that I have shown that the return under-current is as much due to the impulse of the wind as the Gulf-stream itself.

I am also wholly unable to comprehend how Dr. Carpenter should imagine, because the bottom-temperature of the South Atlantic happens to be lower, and the polar water to lie nearer to the surface in this ocean than in the North Atlantic, that therefore this proves the truth of his theory. This condition of matters is just as consistent with my theory as with his. When we consider the immense quantity of warm surface-water which, as has been proved, is being constantly transferred from the South into the North Atlantic (a quantity which to a large extent is compensated by a cold current from the Antarctic regions), we readily understand how the polar water comes nearer to the surface in the former ocean than in the latter. In fact the whole of the phenomena are just as easily explained upon the principle of under-currents as upon Dr. Carpenter's theory.

Dr. Carpenter lays considerable stress on the important fact established by the 'Challenger' expedition, that the great depths of the sea in equatorial regions are occupied by ice-cold water, while the portion heated by the sun's rays is simply a thin stratum at the surface. It seems to me that it would be difficult to find a fact more hostile to his theory than this. Were it not for this upper stratum of heated water there would be no difference between the equatorial and polar columns, and consequently nothing to produce motion. But the thinner this stratum is the less is the difference, and the less there is to produce motion. I have been favoured by the Hydrographer to the Admiralty with a series of temperature-soundings taken along the equator;

and from these I find that to so small a depth does the super-heating extend, that the surface of the ocean at the equator requires to stand only four and a half feet above that at the poles in order to the ocean being in perfect equilibrium. In this case, if we suppose, in order to constant circulation, that the polar column is kept in excess of the equatorial by the weight of, say, two feet of water, there would then remain only a slope of two and a half feet between the equator and poles.

JAMES CROLL.

LVI. *Verification of an Elliptic Transcendent Identity.*

By J. W. L. GLAISHER, M.A.*

THE identity in question is

$$e^{-x^2} + e^{-(x-a)^2} + e^{-(x+a)^2} + e^{-(x-2a)^2} + e^{-(x+2a)^2} + \dots \\ = \frac{2\sqrt{\pi}}{a} \left\{ \frac{1}{2} + e^{-\frac{\pi^2}{a^2}} \cos \frac{2\pi x}{a} + e^{-\frac{4\pi^2}{a^2}} \cos \frac{4\pi x}{a} + \dots \right\}, \quad (1)$$

on which see Cauchy, *Exercices de Mathématiques*, 1827, p. 148; Abel, *Œuvres*, vol. i. p. 308; Schlömilch, *Analytische Studien*, part ii. p. 31; and Sir W. Thomson and Cayley, 'Quarterly Journal of Mathematics,' vol. i. p. 316.

When $x=0$, (1) becomes

$$\frac{1}{2} + e^{-a^2} + e^{-4a^2} + e^{-9a^2} + \dots \\ = \frac{\sqrt{\pi}}{a} \left\{ \frac{1}{2} + e^{-\frac{\pi^2}{a^2}} + e^{-\frac{4\pi^2}{a^2}} + e^{-\frac{9\pi^2}{a^2}} + \dots \right\}, \quad (2)$$

which, expressed symmetrically, takes the form

$$\sqrt[4]{\log \frac{1}{q}} \left(\frac{1}{2} + q + q^4 + q^9 + \dots \right) = \sqrt[4]{\log \frac{1}{r}} \left(\frac{1}{2} + r + r^4 + r^9 + \dots \right),$$

where

$$\log q \cdot \log r = \pi^2.$$

The identity (2), as Abel remarked, is deducible at once from Jacobi's formula,

$$1 + q + q^4 + q^9 + \dots = \frac{1}{2} + \frac{1}{2} \sqrt{\left(\frac{2K}{\pi} \right)};$$

and (1) itself, as was shown by Cayley, is easily obtained by elliptic functions; but the most simple proof of the latter formula is that given by Sir W. Thomson, and which depends on

* Communicated by the Author.

an obvious application of Fourier's theorem to the definite integral

$$\int_0^{\infty} e^{-a^2x^2} \cos bx \, dx = \frac{\sqrt{\pi}}{2a} e^{-\frac{b^2}{4a^2}}.$$

All these methods, however, involve high analytical processes; and as (2) is a very singular identity, it is perhaps worth while to notice how it and (1) admit of being obtained by elementary mathematics, in fact almost by algebra.

From the well-known formula

$$\cot a = \frac{1}{a} + \frac{1}{a-\pi} + \frac{1}{a+\pi} + \frac{1}{a-2\pi} + \frac{1}{a+2\pi} + \dots$$

we deduce by ordinary algebra

$$\begin{aligned} \frac{1}{a^2+x^2} + \frac{1}{a^2+(x-\pi)^2} + \frac{1}{a^2+(x+\pi)^2} + \dots \\ = -\frac{\cot(ai+x) + \cot(ai-x)}{2ai}; \end{aligned}$$

and as

$$\begin{aligned} \cot u &= i \left\{ 1 + \frac{2e^{-ui}}{e^{ui} - e^{-ui}} \right\} \\ &= -i \{ 1 + 2e^{2ui} + 2e^{4ui} + 2e^{6ui} \dots \}; \end{aligned}$$

therefore

$$-\frac{\cot(ai+x) + \cot(ai-x)}{2ai} = \frac{1}{a} \{ 1 + e^{-2a+2xi} + e^{-2a-2xi} + \dots \},$$

whence

$$\begin{aligned} \frac{1}{a^2+x^2} + \frac{1}{a^2+(x-\pi)^2} + \frac{1}{a^2+(x+\pi)^2} + \dots \\ = \frac{1}{a} \{ 1 + 2e^{-2a} \cos 2x + 2e^{-4a} \cos 4x + \dots \}; \quad (3) \end{aligned}$$

herein write $a^2 = nc^2$, and there results

$$\begin{aligned} \frac{1}{n + \left(\frac{x}{c}\right)^2} + \frac{1}{n + \left(\frac{x-\pi}{c}\right)^2} + \frac{1}{n + \left(\frac{x+\pi}{c}\right)^2} + \dots \\ = \frac{c}{\sqrt{n}} \{ 1 + 2e^{-2c\sqrt{n}} \cos 2x + 2e^{-4c\sqrt{n}} \cos 4x + \dots \}. \quad (4) \end{aligned}$$

Now differentiate both sides of this identity $n-1$ times, and then make n infinite. We have

$$\begin{aligned} \left(-\frac{d}{dn}\right)^{n-1} \frac{1}{n + \frac{x^2}{c^2}} &= \frac{1 \cdot 2 \dots (n-1)}{\left(n + \frac{x^2}{c^2}\right)^n} \\ &= \Gamma n \cdot n^{-n} \cdot e^{-\frac{x^2}{c^2}} \\ &= \sqrt{\frac{2\pi}{n}} e^{-n} e^{-\frac{x^2}{c^2}}; \end{aligned}$$

since, n being large, $\Gamma(n+1) = \sqrt{2n\pi} \cdot n^n \cdot e^{-n}$. Thus we have shown that

$$\begin{aligned} e^{-\frac{x^2}{c^2}} + e^{-\left(\frac{x-\pi}{c}\right)^2} + e^{-\left(\frac{x+\pi}{c}\right)^2} + \dots \\ = ce^n \sqrt{\frac{n}{2\pi}} \left(-\frac{d}{dn}\right)^{n-1} \left\{ \frac{1}{\sqrt{n}} + 2 \frac{e^{-2c\sqrt{n}}}{\sqrt{n}} \cos 2x + \dots \right\}; \end{aligned}$$

so that it only remains to find the value of

$$\left(-\frac{d}{dn}\right)^{n-1} \frac{e^{-b\sqrt{n}}}{\sqrt{n}},$$

viz. of

$$\begin{aligned} \left(-\frac{d}{dn}\right)^{n-1} \left\{ n^{-\frac{1}{2}-b} + \frac{b^2}{1 \cdot 2} n^{\frac{1}{2}} - \frac{b^3}{1 \cdot 2 \cdot 3} n + \dots \right\} \\ = \frac{1 \cdot 3 \dots (2n-3)}{2^{n-1}} \cdot n^{-n+\frac{1}{2}} \left\{ 1 - \frac{b^2}{1 \cdot 2} \frac{n}{2n-3} \right. \\ \left. + \frac{b^4}{1 \cdot 2 \cdot 3 \cdot 4} \frac{1 \cdot 3 \cdot n^2}{(2n-3)(2n-5)} + \dots \right\} \\ = \frac{\Gamma(2n+1)}{2^{2n} n \Gamma(n+1)} n^{-n+\frac{1}{2}} \left\{ 1 - \frac{b^2}{4} + \frac{b^4}{1 \cdot 2 \cdot 4^2} - \dots \right\} \\ = \frac{2^{2n+\frac{1}{2}} n^{2n+\frac{1}{2}} e^{-2n} \sqrt{2\pi}}{2^{2n} n^{n+\frac{3}{2}} e^{-n} \sqrt{2\pi}} n^{-n+\frac{1}{2}} \cdot e^{-\frac{b^2}{4}} \\ = \sqrt{\frac{2}{n}} \cdot e^{-n} \cdot e^{-\frac{b^2}{4}}; \end{aligned}$$

whence

$$\begin{aligned} e^{-\frac{x^2}{c^2}} + e^{-\left(\frac{x-\pi}{c}\right)^2} + e^{-\left(\frac{x+\pi}{c}\right)^2} + \dots \\ = \frac{c}{\sqrt{\pi}} (1 + 2e^{-c^2} \cos 2x + e^{-4c^2} \cos 4x + \dots), \end{aligned}$$

which, when transformed by writing x for $\frac{x}{c}$ and a for $\frac{\pi}{c}$, becomes (1).

It is interesting to observe in the above investigation how the

uneven powers of c vanish through the differentiation; so that although the right-hand side of (4) (omitting the factor c , which is unaltered by differentiation), viz.

$$\frac{1}{\sqrt{n}} \{1 + 2e^{-2c\sqrt{n}} \cos 2x + 2e^{-4c\sqrt{n}} \cos 4x + \dots\},$$

involves both even and uneven powers of c , the expression derived from it by differentiation, viz.

$$\frac{1}{\sqrt{\pi}} \{1 + 2e^{-c^2} \cos 2x + e^{-4c^2} \cos 4x + \dots\},$$

is only a function of c^2 .

The identity (1) could be just as well deduced from the well-known equation

$$\frac{\pi}{2a} \frac{e^{a(\pi-x)} + e^{-a(\pi-x)}}{e^{a\pi} - e^{-a\pi}} = \frac{1}{2a^2} + \frac{\cos x}{1^2 + a^2} + \frac{\cos 2x}{2^2 + a^2} + \dots \quad (5)$$

as from (3); but the method adopted has the advantage that it is much easier to prove (3) in an elementary way than (5). In fact, although (5) is demonstrated at once by Fourier's theorem, it does not admit of being established very simply by ordinary algebra (see De Morgan's 'Diff. and Int. Calc.' p. 668). It will be observed that in the proof given above, with the exception of the series for the cotangent, nothing higher than algebra is required, as the continued differentiation may, as usual, be replaced by a comparison of coefficients.

The principle of the process employed consists in operating with the symbol $\left(-\frac{d}{dn}\right)^{n-1}$ and using the two lemmas

$$\left(-\frac{d}{dn}\right)^{n-1} \frac{e^{-b\sqrt{n}}}{\sqrt{n}} = \sqrt{\frac{2}{n}} \cdot e^{-n} \cdot e^{-\frac{b^2}{4}} \quad \dots \quad (6)$$

and

$$\left(-\frac{d}{dn}\right)^{n-1} \frac{1}{n+a} = \sqrt{\frac{2\pi}{n}} \cdot e^{-n} \cdot e^{-a}, \quad \dots \quad (7)$$

n being infinite. The first lemma may also be written

$$\left(-\frac{d}{dn}\right)^n e^{-b\sqrt{n}} = \frac{b}{\sqrt{2n}} \cdot e^{-n} \cdot e^{-\frac{b^2}{4}}.$$

In the 'Messenger of Mathematics' for September 1872 I applied the formulæ (6) and (7) to deduce the integral

$$\int_0^\infty e^{-a^2 x^2} \cos bx \, dx = \frac{\sqrt{\pi}}{2a} e^{-\frac{b^2}{4a^2}} \quad \dots \quad (8)$$

from

$$\int_0^\infty \frac{\cos bx}{a^2 + x^2} dx = \frac{\pi}{2a} e^{-ab};$$

and it is evident that the same formulæ enable us to connect together integrals that are very dissimilar in form. Thus in

$$\int_0^\infty e^{-cx} \cos bx \, dx = \frac{c}{c^2 + b^2};$$

take $c^2 = n$ and we have

$$\int_0^\infty \frac{e^{-x\sqrt{n}}}{\sqrt{n}} \cos bx \, dx = \frac{1}{n + b^2};$$

operate with $\left(-\frac{d}{dn}\right)^{n-1}$, and there results from (6) and (7),

$$\int_0^\infty \sqrt{\frac{2}{n}} \cdot e^{-n} \cdot e^{-\frac{x^2}{4}} \cos bx \, dx = \sqrt{\frac{2\pi}{n}} \cdot e^{-n} \cdot e^{-b^2},$$

viz.

$$\int_0^\infty e^{-\frac{x^2}{4}} \cos bx \, dx = \sqrt{\pi} \cdot e^{-b^2},$$

which is (8).

Or we may use (6) and (7) to deduce the values of complicated integrals from others of a very elementary character: thus

$$\begin{aligned} \int_0^\infty \frac{dx}{x^4 + nx^2 + a^2} &= \frac{1}{\beta - \alpha} \left\{ \int_0^\infty \frac{dx}{x^2 + \alpha} - \int_0^\infty \frac{dx}{x^2 + \beta} \right\} \\ &= \frac{1}{\beta - \alpha} \cdot \frac{\pi}{2} \cdot \left(\frac{1}{\sqrt{\alpha}} - \frac{1}{\sqrt{\beta}} \right) \\ &= \frac{\pi}{2\sqrt{\alpha\beta}} \cdot \frac{1}{\sqrt{\alpha} + \sqrt{\beta}} = \frac{\pi}{2a} \cdot \frac{1}{\sqrt{(n+2a)}}, \end{aligned}$$

since $\alpha + \beta = n$, $\alpha\beta = a^2$. Operate with $\left(-\frac{d}{dn}\right)^{n-1}$, and we have

$$\begin{aligned} \int_0^\infty \frac{dx}{x^2} \cdot e^{-x^2 - \frac{a^2}{x^2}} \cdot \sqrt{\frac{2\pi}{n}} \cdot e^{-n} &= \frac{\pi}{2a} \cdot \frac{1}{2} \cdot \frac{3}{2} \cdots \left(n - \frac{3}{2}\right) \frac{1}{(n+2a)^{n-\frac{1}{2}}} \\ &= \frac{\pi}{2a} \cdot \frac{1 \cdot 3 \cdots (2n-3)}{2^{n-1} \cdot n^{n-\frac{1}{2}}} e^{-2a} \\ &= \frac{\pi}{2a} \frac{\Gamma(2n+1)}{2^{2n} \cdot n \Gamma(n+1)} n^{-n+\frac{1}{2}} e^{-2a} \\ &= \frac{\pi}{2a} \frac{\sqrt{4\pi n} \cdot (2n)^{2n} \cdot e^{-2n}}{2^{2n} \cdot n \cdot \sqrt{(2\pi n)} \cdot n^n e^{-n}} \cdot n^{-n+\frac{1}{2}} \cdot e^{-2a} \\ &= \frac{\pi}{a\sqrt{2n}} e^{-n} \cdot e^{-2a}, \end{aligned}$$

whence

$$\int_0^{\infty} \frac{dx}{x^2} \cdot e^{-x^2 - \frac{a^2}{x^2}} = \frac{\pi}{2a} e^{-2a};$$

and taking $x = \frac{a}{x}$,

$$\int_0^{\infty} dx \cdot e^{-x^2 - \frac{a^2}{x^2}} = \frac{\pi}{2} e^{-2a},$$

the well-known result.

It is scarcely necessary to remark that from (6) we at once derive

$$\left(-\frac{d}{dn}\right)^{n-1} \frac{\cos b \sqrt{n}}{\sqrt{n}} = \sqrt{\frac{2}{n}} \cdot e^{-n} \cdot e^{\frac{b^2}{4}},$$

a formula, however, which for obvious reasons cannot often admit of useful application to definite integrals.

The elliptic-function identities that are given in the *Fundamenta Nova* would, as a rule, be very difficult to verify in an elementary manner; and the facility with which results of a purely algebraical character, but which seem to transcend the ordinary methods of algebra, are established affords one of the most obvious illustrations of the great power of elliptic functions as a branch of analysis.

Of course some of the identities, such as those on p. 103 of the *Fundamenta Nova*, are very easy to prove by mere expansion; but there is a very interesting one of the more difficult kind which was verified by Gauss.

This verification is to be found under the title ‘*Zur Theorie der neuen Transscendenten*,’ *Werke*, vol. iii. p. 446; and the paper containing it consists of a collection of seventy-three formulæ relating to elliptic functions (or, as they are there described, relating to the arithmetico-geometric mean), with here and there a few words of directions, and is rather the materials for a memoir than a memoir itself. It was taken from a note-book of Gauss’s, “finished April 28, 1809,” and first published after his death among his collected works in 1866.

At the bottom of p. 447, immediately following fourteen formulæ of a different kind, there occur the following:—

“Anderer Beweis

$$\begin{aligned} \text{“ 15. } (1-2x+2x^4-\dots)(1+2x+2x^4+\dots) \\ = (1-2x^2+2x^8-\dots)^2. \end{aligned}$$

$$\begin{aligned} \text{“ 16. } (1-2x+2x^4-\dots)^2 + (1+2x+2x^4+\dots)^2 \\ = 2(1+2x^2+2x^8+\dots)^2. \end{aligned}$$

$$\text{"17. } (1+2x+2x^4+\dots)(x^{\frac{1}{4}}+x^{\frac{9}{4}}+\dots)=(x^{\frac{1}{2}}+x^{\frac{9}{2}}+\dots)^2.$$

$$\text{"18. } (1+2x+2x^4+\dots)^2+(2x^{\frac{1}{4}}+2x^{\frac{9}{4}}+\dots)^2 \\ = (1+2x^{\frac{1}{2}}+2x^2+\dots)^2.$$

$$\text{"19. } (1+2x+2x^4+\dots)^4=(1-2x+2x^4-\dots)^4 \\ + (2x^{\frac{1}{4}}+2x^{\frac{9}{4}}+\dots)^4."$$

But for the word *Beweis* at the beginning, it would appear as if this was merely a list of five equations allied to one another in form; and even with the heading it is not easy to see at once what the property proved is, or how the proof is effected. The identity, however, the mode of demonstration of which is sketched in these formulæ, is the elegant theorem 19, which results from substituting for k and k' in terms of q in $k^2+k'^2=1$; and the four formulæ 15-18 are subsidiary results required in the process. As an algebraical demonstration of 19 is valuable, I proceed to expand the proof the steps of which are thus briefly indicated by Gauss in the above extract.

The truth of 16 is very readily seen; for all the terms on the left-hand side involving an exponent which is the sum of an even and an uneven square vanish, while the sum of two even squares

$$4m^2+4n^2=2\{(m+n)^2+(m-n)^2\},$$

and the sum of two uneven squares

$$(2m+1)^2+(2n+1)^2=2\{(m+n+1)^2+(m-n)^2\},$$

which together give the doubles of the sums of all pairs of squares; and the accuracy of the coefficients is evident on consideration.

Formula 16 being thus established, for brevity write

$$\alpha = 1+2x^4+2x^{16}+\dots,$$

$$\beta = 2x+2x^9+2x^{25}+\dots;$$

and let α_1 and β_1 represent respectively α and β with x^2 written for x ; then 16 is

$$(\alpha-\beta)^2+(\alpha+\beta)^2=2(\alpha_1+\beta_1)^2,$$

or

$$\alpha^2+\beta^2=(\alpha_1+\beta_1)^2. \quad \dots \quad (A)$$

Herein write $x\sqrt{-1}$ for x , and β^2 becomes $-\beta^2$, so that

$$\alpha^2-\beta^2=(\alpha_1-\beta_1)^2, \quad \dots \quad (B)$$

which, writing $\alpha^2-\beta^2$ as $(\alpha-\beta)(\alpha+\beta)$, is 15.

By subtraction of (B) from (A),

$$\beta^2=2\alpha_1\beta_1,$$

which, on writing $x^{\frac{1}{2}}$ for x , becomes 17.

By addition of (A) and (B),

$$\alpha^2 = \alpha_1^2 + \beta_1^2,$$

which, on writing $x^{\frac{1}{2}}$ for x , is 18.

We are now in the position to prove 19; for

$$\begin{aligned} (1 + 2x + 2x^4 + \dots)^4 - (1 - 2x + 2x^4 - \dots)^4 \\ &= \{(\alpha + \beta)^2 + (\alpha - \beta)^2\} \{(\alpha + \beta)^2 - (\alpha - \beta)^2\} \\ &= 8(\alpha_1 + \beta_1)^2 \alpha \beta \quad \text{by (A)} \\ &= 16\{1 + 2x^2 + 2x^8 + \dots\}^2 \{x^{\frac{1}{2}} + x^{\frac{9}{2}} + x^{\frac{25}{2}} + \dots\}^2, \end{aligned}$$

on substituting for α_1, β_1 their values, and using 17 with x^4 written for x , which transforms $\frac{1}{2}\alpha\beta$ into the second squared factor,

$$= 16(x^{\frac{1}{4}} + x^{\frac{9}{4}} + x^{\frac{25}{4}} + \dots)^4,$$

by applying 17 again, x^2 being therein written for x ; and the formula 19 is thus established by elementary algebra in a most simple and elegant manner.

The formula 18 is not required in the proof, but was no doubt added by Gauss for the sake of completeness.

Trinity College, Cambridge,

April 25, 1874.

LVII. On the Mathematical Theory of Isomers.

By Professor CAYLEY, F.R.S.*

I CONSIDER a "diagram," viz. a set of points H, O, N, C, &c. (any number of each), connected by links into a single assemblage under the condition that through each H there passes not more than one link, through each O not more than two links, through each N not more than three links, through each C not more than four links. Of course through every point there passes at least one link, or the points would not be connected into a single assemblage.

In such a diagram each point having its full number of links is saturate, or nilvalent: in particular each point H is saturate. A point not having its full number of links is univalent, bivalent, or trivalent, according as it wants one, two, or three of its full number of links. If every point is saturate the diagram is saturate, or nilvalent; or, say, it is a "plerogram;" but if the diagram is susceptible of n more links, then it is n -valent; viz. the valency of the diagram is the sum of the valencies of the component points.

Since each H is connected by a single link (and therefore to

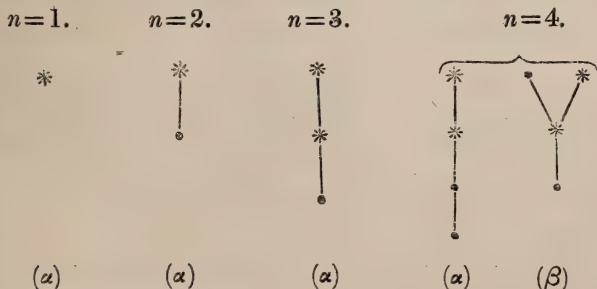
* Communicated by the Author.

a point O, C, &c. as the case may be, but not to another point H), we may without breaking up the diagram remove all the points H with the links belonging to them, and thus obtain a diagram without any points H: such a diagram may be termed a "kenogram:" the valency is obviously that of the original diagram *plus* the number of removed H's.

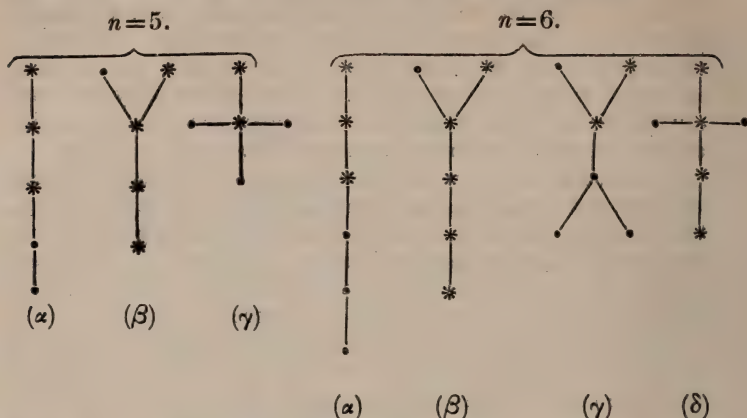
If from a kenogram we remove every point O, C, &c. connected with the rest of the diagram by a single link only (each with the link belonging to it), and so on indefinitely as long as the process is practicable, we arrive at last at a diagram in which every point O, C, &c. is connected with the rest of the diagram by two links at least: this may be called a "mere kenogram."

Each or any point of a mere kenogram may be made the origin of a "ramification;" viz. we have here links branching out from the original point, and then again from the derived points, and so on any number of times, and never again uniting. We can thus from the mere kenogram obtain (in an infinite variety of ways) a diagram. The diagram completely determines the mere kenogram; and consequently two diagrams cannot be identical unless they have the same mere kenogram. Observe that the mere kenogram may evanesce altogether; viz. this will be the case if the diagram or kenogram is a simple ramification.

A ramification of n points C is $(2n+2)$ -valent: in fact this is so in the most simple case $n=1$; and admitting it to be true for any value of n , it is at once seen to be true for the next succeeding value. But no kenogram of points C is so much as $(2n+2)$ -valent; for instance, 3 points C linked into a triangle, instead of being 8-valent are only 6-valent. We have therefore plerograms of n points C and $2n+2$ points H, say plerograms $C H^{2n+2}$; and in any such plerogram the kenogram is of necessity a ramification of n points C; viz. the different cases of such ramifications are*



* The distinction in the diagrams of asterisks and dots is to be in the first instance disregarded; it is made in reference to what follows, the explanation as to the allotrious points.



where the mathematical question of the determination of such forms belongs to the class of questions considered in my paper "On the Theory of the Analytical Forms called Trees," *Phil. Mag.* vol. xiii. (1857) and vol. xv. (1859), and in some papers on Partitions in the same Journal.

The different forms of univalent diagrams $C^n H^{2n+1}$ are obtained from the same ramifications by adding to each of them all but one of the $2n+2$ points H ; that is, by adding to each point C except one its full number of points H , and to the excepted point one less than the full number of points H . The excepted point C must therefore be univalent at least; viz. it cannot be a saturate point, which presents itself for example in the diagrams $n=5(\gamma)$ and $n=6(\delta)$. And in order to count the number of distinct forms (for the diagrams $C^n H^{2n+1}$), we must in each of the above ramifications consider what is the number of distinct classes into which the points group themselves, or, say, the number of "allotrious" points. For instance, in the ramification $n=3$ there are two classes only; viz. a point is either terminal or medial; or, say, the number of allotrious points is $=2$: this is shown in the diagrams by means of the asterisks; so that in each case the points which may be considered allotrious are represented by asterisks, and the number of asterisks is equal to the number of allotrious points.

Thus, number of univalent diagrams $C^n H^{2n+1}$:—

$n=1,$		1
$n=2,$		1
$n=3,$		2
$n=4,$	(α) 2; (β) 2; together	4
$n=5,$	(α) 3; (β) 4; (γ) 1; „	8
$n=6,$	(α) 3; (β) 5; (γ) 2; (δ) 3; „	13

where it will be observed that, $n=5$ (γ), and $n=6$ (δ), the numbers of allotropic points are 2 and 4 respectively; but since in each of these cases one point is saturate, they give only the numbers 1 and 3 respectively. It might be mathematically possible to obtain a general solution; but there would be little use in this; and for even the next succeeding case, No. of bivalent diagrams $C^n H^{2n}$; the extreme complexity of the question would, it is probable, prevent the attainment of a general solution.

Passing to the chemical signification of the formulæ, and instead of the radicals $C^n H^{2n+1}$ considering the corresponding alcohols $C^n H^{2n+1}.OH$, then, $n=1, 2, 3, 4$, the numbers of known alcohols are 1, 1, 2, 4, agreeing with the foregoing theoretic number (see Schorlemmer's 'Carbon Compounds,' 1874); but $n=4$, the number of known alcohols is =2, instead of the foregoing theoretic number 8. It is, of course, no objection to the theory that the number of theoretic forms should exceed the number of known compounds; the missing ones may be simply unknown; or they may be only capable of existing under conceivable, but unattained, physical conditions (for instance, of temperature); and if defect from the theoretic number of compounds can be thus accounted for, the theory holds good without modification. But it is also possible that the diagrams, in order that they may represent chemical compounds, may be subject to some as yet undetermined conditions; viz. in this case the theory would stand good as far as it goes, but would require modification.

LVIII. *Proceedings of Learned Societies.*

ROYAL SOCIETY.

[Continued from p. 391.]

December 11, 1873.—Joseph Dalton Hooker, C.B., President, in the Chair.

THE following communication was read:—

“A Quantitative Investigation of certain Relations between the Gaseous, the Liquid, and the Solid States of Water-Substance.” By Professor James Thomson, LL.D., Queen's College, Belfast.

In two communications made by me to the British Association, at its Meetings at Edinburgh in 1871 and at Brighton in 1872, and printed as abstracts in the Transactions of the Sections for those years, considerations were adduced on relations between the gaseous, the liquid, and the solid states of matter. The new subject of the present paper constitutes a further development of some of those previous considerations; and a brief sketch of these

is necessary here as an introduction for rendering intelligible what is to follow.

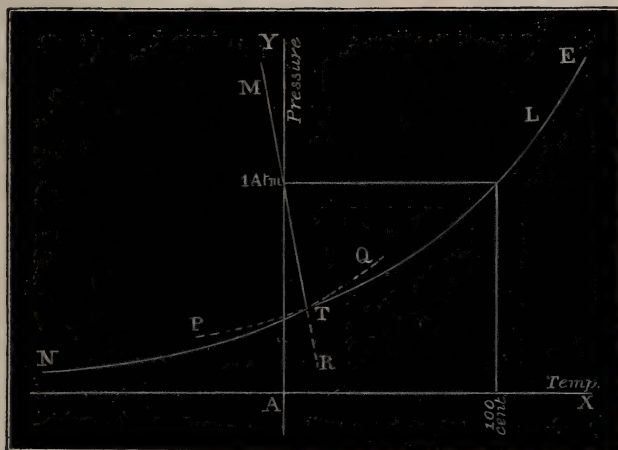
Taking into consideration any substance which we can have in the three states, gaseous, liquid, and solid, we may observe that, when any two of these states are present in contact together, the pressure and temperature are dependent each on the other, so that when one is given the other is fixed. Then, if we denote geometrically all possible points of temperature and pressure jointly by points spread continuously in a plane surface, each point in the plane being referred to two axes of rectangular coordinates, so that one of its ordinates shall represent the temperature and the other the pressure denoted by that point, we may notice that there will be three curves—one expressing the relation between temperature and pressure for gas with liquid, another expressing that for gas with solid, and another expressing that for liquid with solid. These three curves, it appears, must all meet or cross each other in one point of pressure and temperature jointly, which may be called the triple point*.

The curve between gas and liquid, which may be called the *boiling-line*, will be a separating boundary between the regions of the plane corresponding to the ordinary liquid and those corresponding to the ordinary gaseous state. But by consideration of Dr. Andrews's experimental results (Phil. Trans. 1869), we may see that this separating boundary comes to an end at a point of tempera-

* In making this statement, that it appears that the three curves must all cross each other in one point, I would wish to offer it here (as I previously did in the 1871 British-Association paper) subject to some reserve in respect of conditions not yet known with perfect clearness and certainty. I have to suggest that we might not be quite safe in assuming that, within a cavity containing nothing but pure water-substance partly gaseous, the melting temperature and pressure of ice solidified from the gaseous state would be the same as the melting temperature and pressure of ice frozen from the liquid state, and in making other suppositions, such as that the same quantity of heat would become latent in the melting of equal quantities of ice formed in these two ways, and in neglecting conceivable but, I presume, as yet imperfectly known distinctions of capillary conditions between ice amply wet with water and ice only moistened with the last vestiges of water before the whole liquid may be either evaporated or frozen. It might be a question in like manner whether we can be sure that there can be theoretically a condition of repose in a cavity containing only perfectly pure water-substance in which the three states are present together, each in contact with the other two, so that there would be ice partly wet with water, and partly dry in contact with gaseous water-substance, or steam as it may be called, while the water and steam were also in contact with each other. I offer these remarks by way of caution, as they force themselves into notice when we attempt to sketch out the features of the three curves under consideration, and because they may serve to suggest questions for experimental and theoretical investigation which may have been generally overlooked before. In the present paper, however, I proceed on assumptions, such as are usually tacitly made, of identity in the thermal and dynamic conditions of pure ice solidified in different ways, assumptions which, so far as is known, may be, and probably are, perfectly true; and I proceed on the supposition that there can be theoretically the condition of repose here alluded to, of the solid, liquid, and gaseous states, present together each in contact with the other two—and consequently that the three curves would meet or cross each other in one point, which I have called the *triple point*.

ture and pressure which, in conformity with his language, may be called the *critical point* of pressure and temperature jointly; and we may see that, from any liquid state to any gaseous state, the transition may be gradually effected by an infinite variety of courses passing round the extreme end of the boiling-line*.

The accompanying figure serves to illustrate these considerations in reference to transitions between the three states, the gaseous, the liquid, and the solid. The figure is intended only as a sketch to illustrate principles, and is not drawn according to measurements for any particular substance, though the main features of the curves shown in it are meant to relate in a general way to the substance of water, steam, and ice. A X and A Y are the axes of coordinates for temperatures and pressures respectively, A (the origin) being taken as the zero for pressures and as the zero for temperatures on the Centigrade scale. The curve L represents



the *boiling-line* terminating in the critical point E. The line T M represents the line between liquid and solid. It is drawn showing in an exaggerated degree the lowering of the freezing temperature of water by pressure, the exaggeration being necessary to allow small changes of temperature to be perceptible in the diagram. The line T N represents the line between the gaseous and the solid states of water-substance. The line L T N appears to have been generally (in the discussion of experimental results on the pressure of aqueous vapour above and below the freezing-point) regarded as one continuous curve; but it was a part of my object in the two British-Association papers referred to, to show that it ought to be considered two distinct curves (L T P and N T Q) crossing each other in the triple point T.

In the second of the two British-Association papers already re-

* Mention of this condition has been already made in a former paper by me, in the 'Proceedings of the Royal Society,' November 16, 1871, page 2.

ferred to (the one read at the Brighton Meeting, 1872), I gave demonstrations showing that these two curves L T and N T should meet, as shown in the accompanying figure, with a re-entrant angle at T, not with a salient angle such as is exemplified in the vertex of a pointed arch, and offered in conclusion the suggestion that the reasoning which had been adduced might be followed up by a quantitative calculation founded on experimental data, by which calculation the difference of the pressures of steam with water and steam with ice for any given temperature very near the triple point may be found with a very close approximation to the truth.

In the month of last October (October 1872) I explained to my brother, Sir William Thomson, the nature of that contemplated quantitative calculation : I mentioned to him the method which I had prepared for carrying out the intended investigation, and inquired of him for some of the experimental data, or data already deduced by theory from experiments, which I was seeking to obtain. On his attention being thus turned to the matter, he noticed that the desired quantitative relation could be obtained very directly and easily from a simple formula which he had given in his paper on the Dynamical Theory of Heat, Transactions of the Royal Society of Edinburgh, March 17, 1851, § 21 (3), to express the second law of thermodynamics for a body of uniform temperature throughout, exposed to pressure equal in all directions.

That formula is

$$\frac{dp}{dt} = C M ;$$

in which p denotes the amount of the pressure, and $\frac{dp}{dt}$ its rate of increase per unit increase of temperature, the volume being kept constant ; C denotes Carnot's function ; and M denotes the rate of absorption at which heat must be supplied to the substance per unit augmentation of volume, to let it expand without varying in temperature. The body may be either homogeneous throughout, as a continuous solid, or liquid, or gas ; or it may be heterogeneous, as a mass of water and aqueous vapour (*i. e.* steam), or ice and water, or ice and aqueous vapour (*i. e.* steam).

Now apply that formula, 1st, to steam with water, and, 2nd, to steam with ice, the temperature of the heterogeneous body in each case being that of the triple point ; or we may, for the present purpose, say 0° Centigrade, which is almost exactly the same. It is to be observed that while in the general application of the formula the rate of increase of the pressure with increase of temperature, when the volume is kept constant, has been denoted by $\frac{dp}{dt}$, yet in each of the two particular cases now brought under consideration, it is a matter of indifference whether the volume be kept constant or not ; because the pressure of steam in contact either with water or with ice, for any given temperature, is independent of the volume of the whole heterogeneous body ; so that the change of

pressure for change of temperature is independent of whether there be change of volume or not. As C is a function of the temperature which has the same value for all substances at the same temperature, it has the same value for the two cases now under consideration. Hence, retaining for the first case (that, namely, of steam with water) the same notation as before, but modifying it by the use of an accent where distinction is necessary in the second case (that of steam with ice), and thus using $\frac{dp}{dt}$ to denote

the rate of increase of the pressure per unit increase of temperature for steam with water at the triple point (0° Centigrade nearly), and M to denote the rate of absorption at which heat must be supplied to a body consisting of steam and water at the triple point, per unit augmentation of volume of that whole heterogeneous body, to let it expand without varying in temperature, and using $\frac{dp'}{dt'}$ and M' to denote the corresponding rates for steam with ice at the triple point, we have

$$\frac{\frac{dp}{dt}}{\frac{dp'}{dt}} = \frac{M}{M'}.$$

The latent heat of evaporation of one pound of water at the freezing-point (or triple point) into steam at the same temperature, as determined by Regnault, is 606.5 thermic units, the thermic unit being here taken as the heat which would raise the temperature of one pound of water one degree Centigrade; and the latent heat of fusion of ice is about 78 or 79 of the same thermic units. Hence, though M and M' belong each to a cubic foot of steam at the triple point, not to a pound mass of it, still the ratio $\frac{M}{M'}$ is

$$= \frac{606}{79 + 606}.$$

Hence

$$\frac{\frac{dp}{dt}}{\frac{dp'}{dt}} = \frac{606}{79 + 606} = \frac{1}{1.13}.$$

This shows that for any small descent in temperature from the triple point (where the pressure of steam with ice is the same as that of steam with water), the pressure of steam with ice falls off 1.13 times as much as does the pressure of steam with water.

In submitting the quantitative calculation now given, I have preferred to adopt the method proposed and developed by my brother rather than that which I had myself previously devised, because his method is simpler, and brings out the results more briefly by estab-

lished principles from existing experimental data. I may say, however, that the method devised by myself was also a true method, and that I have since worked it out to its numerical results, and have found that these are quite in accordance with those brought out by my brother. The two indeed may be regarded as being essentially of the same nature; and I think it unnecessary to occupy space by giving any details of the method I planned and have carried out. Its general character may be sufficiently gathered from the concluding passages of the British-Association 1872 paper, as printed in the Transactions of the Sections, Brighton Meeting.

In order to discover whether the feature now developed by theoretical considerations is to be found showing itself in any degree in the experimental results of Regnault on the pressures of steam at different temperatures*, I have made careful examinations of his engraved curve (plate viii. of his memoir), and of his empirical formulæ adapted to fit very closely to the results exhibited in that curve, and of his final Tables of results at the close of his memoir; and by every mode of scrutiny which I have brought to bear on the subject (in fact by each of some seven or eight varied modes) I have met with clear indication of the existence of the expected feature; and by some of them I have found that it can readily be brought prominently into notice. The engraved curve drawn on the copper plate by Regnault himself is offered by him as the definitive expression of his experiments, as being an expression which satisfies as well as possible the aggregate of his observations—subject, however, to a very slight alteration, which he has pointed out as a requisite amendment in the part of the curve immediately below the freezing-point, a part with which the investigations in the present paper are specially concerned.

After telling (page 581) of the great care with which he had marked the curve on the copper plate and got it engraved, he says :—“ Je n’ai pas pu éviter cependant quelques petites irrégularités dans les courbes ; mais une seule de ces irrégularités me paraît assez importante pour devoir être signalée. Elle se présente pour les basses températures comprises entre 0° et -16° ; la courbe creuse trop vers l’axe des températures, elle laisse, notablement au-dessus d’elle, toutes les déterminations expérimentales qui ont été faites entre 0° et -10° . Ainsi les valeurs, que cette petite portion de la courbe donne pour les forces élastiques, sont un peu trop faibles, et j’ai eu soin de les augmenter, de la quantité convenable, dans les nombres que je donnerai plus loin.” Whether we are now to think that this bend downwards† of the curve towards the axis of temperatures, involving what Regnault regarded as a small faulty departure of his drawn curve from his actual experiments, was introduced merely by a casual want of accuracy in drawing,

* Regnault, “ Des Forces Élastiques de la Vapeur d’Eau aux différentes Températures,” Mémoires de l’Académie des Sciences, 1847.

† In M. Regnault’s curve the temperatures are measured horizontally across the sheet, and pressures are measured upwards.

or whether we may suppose that possibly there may have been some experimental observations which attracted the curve downwards, but were afterwards rejected on a supposition of their being untrustworthy, it appears that such a bend is a feature which the curve really ought to possess, and is one which even after being partially smoothed off by way of correction is not obliterated, but still remains clearly discoverable in the final numerical tables of results.

This is best brought to light by means of the empirical formulæ devised and employed by Regnault for the collating of his results. He proceeded evidently under the idea of the curve being continuous in its nature, so that a single formula might represent the pressures of aqueous vapour throughout the whole of his experiments; but before seeking for such a formula he proceeded to calculate several local formulæ of which each should represent very exactly his experiments between limits of temperature not wide apart; and afterwards he worked out several general formulæ, each adapted singly for the whole range of his experiments.

In regard to the one of these general formulæ which he designates as formula (H)*, he says that it represents the aggregate of his determinations of the pressures of the vapour of water, referred to the air-thermometer, and extending between the extreme temperatures of -33° and $+232^{\circ}$ with such precision that there could not be any hope of attaining to representing them better by any other mode of interpolation, because the differences, he says, between the calculated numbers and the numbers deduced from his graphic constructions are always smaller than the probable errors of observation. Still, for making out his final general Table of pressures of steam for every degree of the air-thermometer from -30° to $+230^{\circ}$, he used three local formulæ, finding that by them he could get slightly closer agreements with his experimental determinations than by using the single formula (H) for the whole range. Thus between -32° and 0° he used his formula designated as (E); from 0° to 100° he used his formula (D); and between 100° and 230° he used his formula (H). He points out (page 623) that he might have calculated this Table throughout its entire extent by the single formula (H), and that he would thus have got almost identically the same values by it from 100° down to 40° as those he calculated by the formula (D), but that between $+40^{\circ}$ and -20° the pressures given by the formula (H) would be slightly too small. This gives indication of the existence of the feature which it is my object at present to bring into view; and an examination of the column of Differences in Regnault's Table on his page 608, adapted for comparing the pressures got from experiments as

* This and other formulæ in M. Regnault's memoir are here referred to only by their letters of reference, because to cite the formulæ themselves with their necessary accompanying explanations, would extend the present paper to too great a length; and any person wishing to scrutinize the formulæ would naturally prefer to have recourse to the original memoir.

expressed by his graphic curve with those got from the formula (H), shows distinctly a re-entrant angle, or at least a flattened place, in the curve at or about 0° . Several other like comparisons, by means of his other formulæ, give like indications; but most of these may for brevity be passed over without further mention here. The most decisive indication comes out in the following way. We may observe that for temperatures adjacent to the freezing-point and extending both ways from it, Regnault finally adopted as fitting best to his experiments the formula (E) for temperatures descending from 0° , and the formula (D) for temperatures ascending from 0° . He tried (at pages 598, 599 of his memoir) the continuing of the application of his formula (D) beyond the inferior of the two limits 0° and 100° , for which he had specially aimed at adapting it to his experimental determinations; and he found that in calculating by it the pressures which it would give for temperatures below 0° , these pressures come out always slightly in excess of those which were given by his experiments. I have developed this mode of comparison in a more complete manner, and have arrived at remarkable results. The formula (D) may be regarded as the formula for giving the pressure p of steam with water, and (E) as that for giving the pressure p' for steam with ice. The following two Tables show the pressures p and p' for temperatures, in each case, both below and above the freezing-point, as calculated from these two formulæ; and they show, also in each case, the consequent differences of pressure for 1° change of temperature at several different temperatures, or, what is the same, the values of $\frac{dp}{dt}$ and $\frac{dp'}{dt}$ for several temperatures slightly above and slightly below the freezing-point.

TABLE I. By Formula (D); Steam with Water.

Temperatures.	Pressures = p .	Differences for 1° , which are values of $\frac{dp}{dt}$ for intermediate temperatures.	Temperatures to which the values of $\frac{dp}{dt}$ belong.
-3°	3.703	} .280 } .298 } .319 } .340 } .362 } .385	$-2\frac{1}{2}^{\circ}$
-2°	3.983		$-1\frac{1}{2}^{\circ}$
-1°	4.281		$-\frac{1}{2}^{\circ}$
0°	4.600		$+\frac{1}{2}^{\circ}$
$+1^{\circ}$	4.940		$+1\frac{1}{2}^{\circ}$
$+2^{\circ}$	5.302		$+2\frac{1}{2}^{\circ}$
$+3^{\circ}$	5.687		

TABLE II. By Formula (E) ; Steam with Ice.

Temperatures.	Pressures = p' .	Differences for 1° , which are values of $\frac{dp'}{dt}$ for intermediate temperatures.	Temperatures to which the values of $\frac{dp'}{dt}$ belong.
-3°	3.644	} .297 } .322 } .347 } .375 } .405 } .437	$-2\frac{1}{2}^\circ$
-2°	3.941		$-1\frac{1}{2}^\circ$
-1°	4.263		$-\frac{1}{2}^\circ$
0°	4.610		$+\frac{1}{2}^\circ$
$+1^\circ$	4.985		$+1\frac{1}{2}^\circ$
$+2^\circ$	5.390		$+2\frac{1}{2}^\circ$
$+3^\circ$	5.827		

From these two Tables we obtain the following values of $\frac{dp'}{dt}$ as deduced from Regnault's formulas (D) and (E).

TABLE III.

Temperatures.	Values deduced for $\frac{dp'}{dt}$ $\frac{dp}{dt}$
$-2\frac{1}{2}^\circ$	$\frac{297}{280} = 1.06$
$-1\frac{1}{2}^\circ$	$\frac{322}{298} = 1.08$
$-\frac{1}{2}^\circ$	$\frac{347}{319} = 1.09$
$+\frac{1}{2}^\circ$	$\frac{375}{340} = 1.10$
$+1\frac{1}{2}^\circ$	$\frac{405}{362} = 1.12$
$+2\frac{1}{2}^\circ$	$\frac{437}{385} = 1.13$

This gives for $\frac{dp'}{\frac{dp}{dt}}$ at the freezing-point the value of about 1.09

or 1.10, while its value brought out in the earlier part of the present paper by my brother's quantitative calculation was 1.13; and so the feature expected shows itself here in Regnault's results almost in the full extent in which theory shows that it ought to exist.

Regnault gives in the same memoir (page 627 and following pages) another Table, one intended chiefly for meteorological purposes, and in which the pressures are stated from -10° to $+35^{\circ}$ for every $\frac{1}{10}$ of a degree. In this Table the numbers inserted as representing the pressures below the freezing-point are slightly different from the corresponding ones in his general Table already referred to; and he mentions that this slight discrepancy has resulted from the fact that the two Tables were formed at different periods, and were not calculated by the same formula; but he remarks that the differences are insignificant, as they scarcely amount to .02 millimetre. Here, too, as in the general Table, the feature expected shows itself, though in a diminished degree. By a careful examination of its column of Differences for $\frac{1}{10}$ of a degree, and by making a few small arithmetical adjustments which may be regarded as amendments in the way of interpolation in that column, I find that, according to the experimental results as they are re-

presented in this Table, the value of $\frac{dp'}{\frac{dp}{dt}}$ at the freezing-point would

come out to be about 1.05 or 1.06. We have seen by the new calculation, based on theory, in the present paper that it ought to be 1.13; so here the feature is found showing itself in about half the degree in which, according to the new quantitative calculation, it ought to be met with. When we consider that Regnault's reductions of his experimental results in the making out of curves, formulæ, and tables for representing them in the aggregate were, as we have sufficient ground to suppose, carried out under the idea, now proved to be erroneous, of there being, for aqueous vapour, continuity in variations of pressure with variations of temperature past the freezing-point, just as past any other point of temperature, and when we further consider that the quantities with which we are here concerned are indeed very small, it is not surprising that there should have been a tendency to smooth off this feature on the supposition that any departures of the experimental observations from the course of a continuous or smooth curve were only slight irregularities due to experimental errors or imperfections.

It may now, in conclusion, be remarked that if from experiments

independent of those which have been made, or may be made, directly on the pressure of aqueous vapour at different temperatures near the freezing-point, both above and below it, very correct determinations of the values of the quantities C , M , and M' can be made, such determinations will lead to more correct evaluations of $\frac{dp}{dt}$ and $\frac{dp'}{dt}$ for aqueous vapour in contact in the one case with liquid water, and in the other with ice, than we at present possess. Such determinations, we may presume further, would, if very trustworthily arrived at, conduce to the attainment of a more correct estimate of the density of steam at the freezing-point (or at the triple point) than we now possess. In fact, in connexion with the subject which has been here under consideration, there are various important quantities so connected that improved determinations of one or more of them may lead to more correct evaluations of others.

GEOLOGICAL SOCIETY.

[Continued from p. 315.]

May 14, 1873.—Joseph Prestwich, Esq., F.R.S., Vice-President, in the Chair.

The following communications were read:—

1. "On the genus *Palæocoryne*, Duncan and Jenkins, and its affinities." By P. Martin Duncan, M.B. Lond., F.R.S., V.P.G.S., Prof. of Geology in King's College, London.

In this paper the author referred to certain minute fossils from the Carboniferous rocks of Scotland, described by himself and Mr. Jenkins in a paper read before the Royal Society, as belonging to the Hydroida, and most nearly resembling the recent genus *Bimeria*, Wright. He stated that numerous specimens since received from the Geological Society of Scotland threw some further light on the nature of these fossils, and showed especially that in all probability the base is not really cellular, but that the cellular appearance is produced by the growth of the real base of the polype over the cells of the *Fenestella* on which it grows. The author further described what he believed to be the mode of growth of this organism, as displayed in the new specimens.

2. "Notes on Structure in the Chalk of the Yorkshire Wolds." By J. R. Mortimer, Esq.

In this paper the author described a peculiar structure observable in chalk from Yorkshire and elsewhere, giving it a striated appearance. This structure had been ascribed by Dr. Mackie and others to slickensides. The author adduced reasons for doubting the mechanical origin of these striations, and argued that they are of an organic nature. He ascribed them to corals, and remarked that similar striæ occur in all limestone formations.

3. "On *Platysium sclerocephalum* and *Palæospinax priscus*, Egerton." By Sir Philip de Malpas Grey-Egerton, Bart., M.P., F.R.S., F.G.S.

The two species of fossil Fishes noticed in this paper were described by the author in the 13th Decade of the Memoirs of the Geological Survey, published in 1872. They are both from the Lias of Lyme Regis. He now described some new specimens which add to our knowledge of their characters. An example of *Platysium* shows the position of the dorsal fin, which is placed very far back, occupying a place opposite to the interval between the ventral and anal fins, and the form of the trunk, which is of nearly uniform depth from the occiput to the base of the dorsal fin. The structure of the dorsal fin was described in detail. The new specimen of *Palæospinax priscus* shows especially the position of the second dorsal spine, which is placed over the 50th vertebra, the first being on the 16th, the fish thus most nearly approaching the existing *Cestracion*, which it also resembles in its dentition. In other respects it seemed to be most nearly allied to *Acanthias*.

4. "On a new genus of Silurian Asteriadae." By Thos. Wright, M.D., F.R.S.E., F.G.S.

The specimen described showed the outline of a small Starfish, with a large disk and short rays, in a slab of Wenlock Limestone from Dudley. The outline of the ten rays was described as marked out by the border of small triangular spines, the other plates of the disk and rays being absent. Each ray was terminated by a stem-like multiarticulate process as long as the ray, from towards the extremity of which spring slender lateral processes, giving it a tufted appearance. This Starfish, which is in the collection of Dr. Grindrod, is named by the author *Trichotaster plumiformis*.

May 28, 1873.—Prof. Ramsay, F.R.S., Vice-President,
in the Chair.

The following communications were read :—

1. "The Glaciation of the Northern part of the Lake-district." By J. Clifton Ward, Esq., F.G.S.

The author stated the leading questions to be settled by his investigation of the northern part of the Lake-district as follows :—The fact of the glaciation of the district being granted, and of this he adduced abundant evidence, the questions that arose were whether the glaciating agent worked from north to south, whether it came from within or from without the district, and finally, whether the agent was floating ice, a system of local glaciers, or an unbroken ice-cap. As the result of his investigation, he maintained that there is no evidence that a great ice-cap from the north ever swept over this district. The ice-scratches trending along the principal valleys, but sometimes crossing watersheds, indicate a great confluent glacier-sheet, at one time almost covering a great part of the district, the movement of which was determined by the principal watershed of the Lake-district. In the part of the Lake-district under

consideration the ice, during its increase, carried forward, from south to north, a great quantity of rocky material. There are no signs in the district of the occurrence of mild periods during the epoch of primary glaciation; but the author thought that the climate had probably become moderate before the great submergence of the land commenced. The author noticed the effect of the submergence upon the results of previous glacial action, and maintained that when the land had sunk 800 or 900 feet there was a recurrence of cold, and boulders were transported by floating ice. Until the submergence reached 1500 feet there was no direct communication between the northern and southern halves of the Lake-district, except by the Straits of Dunmail Raise. From the directions which would be taken by the currents in the sea at this period, it would appear that boulders may then have been transported by floating ice in some of the same directions as they had previously been carried by glacier-ice. The extreme of submergence appeared to have been about 2000 feet. The author further maintained that on the relevation of the district there was a second land-glaciation, affecting the higher valleys and clearing them of marine drift.

2. "*Alluvial and Lacustrine Deposits and Alluvial Records of the Upper Indus Basin.*" By Frederic Drew, Esq., F.G.S.

First stating that the alluvium of that country had been noticed by several travellers, especially by Major Godwin-Austen, who had given much important information about them, the author said that he felt the necessity for a careful classification of the phenomena of alluvial deposits; for the want of recognition of the different kinds was likely to lead to incorrect deductions; the classification he proposed was the following:—

I. *Loosened material*, which consisted of disjointed rocks or loose angular stones, sometimes mixed up with mud, which had been separated and disintegrated, but since that had remained *unmoved*.

II. *Taluses*, the substance of which had fallen by its own weight, and not been transported by streams. These were the great heaps of angular matter that were found at the foot of cliffs, with a slope generally of near 35° . A special form was the fan talus, which occurred where the falling matter had either originated from, or collected to, one spot, from which again it spread, and made a partial cone of the same slope as the ordinary taluses.

III. *Alluvial Fans*.—These were the fan-shaped extensions of alluvial or torrential matter that spread out from the mouths of gorges, where these debouched into a more open valley. They were in form cones of a low angle, commonly 5° or so; they had accumulated by layer after layer on a cone-shaped surface, as shown by the radial sections exhibiting layers of a straight slope, and the chord sections showing curves, which were by the theory hyperbolas. Many complicated phenomena were produced by the denudation of these fans, and the production of secondary ones, some of which were illustrated by diagrams.

IV. *Alluvium*, which was defined as a deposit which sloped down

the direction of the valley of the stream which had made it, and did not appreciably slope or curve over in a direction at right angles to that.

The alluvium of the country in question consisted mostly of pebble-beds, well stratified; it was common at all heights, from 16,000 feet or more downwards. Sections of some 100 feet thick were to be observed at intervals along the valleys of most of the rivers: one of 500 feet was described as occurring near Sumkiel, in Rupshu; and 200, 300, and 400 feet heights of the alluvial terraces above the rivers were very general.

In a few cases there were wider openings, filled with similar deposits. The tableland of Deosai, where the alluvium made flats at an elevation of 12,500 and 13,000 feet, surrounded by a ring of mountains 16,000 and 17,000 feet high, was the most remarkable of these.

A special phenomenon was described as now and then occurring in the alluvium, namely a sloping of the alluvial strata, looking at first like false-bedding on a large scale, and, further, a curving or bending of them till they reached round even beyond the perpendicular. This the author attributed to the ploughing of the foot of a glacier against the alluvium that had been formed in front of it by its own streams.

The last class of alluvial deposits would be the *lacustrine*; but a description of these was reserved for a future portion of the paper.

Summary and Inferences.

There was evidence of a succession of three states:—

1st. The cutting of the valleys.

2nd. The accumulation of alluvial matter.

3rd. The cutting down of the streams through that alluvial matter.

Accumulation denotes an excess of supply of material from the rocks (by disintegration) over what can be carried away by the streams.

Denudation, or the cutting down of the streams through their alluvium (the lowering of their beds), denotes a deficiency of supply of material from the rocks as compared with the transporting power of the streams. Hence the author inferred that the period of great accumulation of these alluvial deposits was one of great disintegration of rocks, one of intense frost (in other words, it was the Glacial period), and that the denudation occurred when the cold lessened and there came to be a smaller supply of disintegrated material.

The connexion of various glacial phenomena with the alluvium, such as the one described above, was taken to corroborate the inference that the greater deposits were made during the Glacial epoch.

June 11, 1873.—Prof. Ramsay, F.R.S., Vice-President, in the Chair.

The following communications were read:—

1. “On the Nature and probable Origin of the superficial Deposits in the Valleys and Deserts of Central Persia.” By W. T. Blanford, Esq., F.G.S.

In this paper the author described the general characters of the superficial deposits of Central Persia, and noticed the physical geography and rainfall of that country. He described especially the desert plains of the interior of the country, the paucity and scantiness of the streams, most of which terminate in salt swamps and lakes, and the occurrence of vast slopes of gravel on the margins of the desert plains, covering up the junction of the latter with the surrounding mountains. The desert plains he regarded as in general the beds of ancient lakes. His general results may be summed up as follows:—Persia has undergone a gradual change from a moister to a drier climate simultaneously with the elevation of portions of its surface, resulting first in the conversion of old river-valleys into enclosed basins containing large lakes, probably brackish or salt. Then, as the rainfall diminished, the lakes gradually dried up, leaving desert plains. The amount of subaërial disintegration among the rocks of the high ground he considered to be in excess of the force available for its removal, the water which now falls only sufficing to wash the loosened materials from the steeper slopes into the valleys; and hence the valleys in the upper parts are gradually being filled up with coarse gravel-like detritus, just as their lower portions have been already hidden beneath lake-deposits.

2. “On *Caryophyllia Bredai*, Milne-Edwards and Haime, from the Red Crag of Woodbridge.” By Prof. P. Martin Duncan, M.B., F.R.S., V.P.G.S.

The author recorded the occurrence in the Red Crag of the Woodbridge district of a variety of *Caryophyllia Bredai*, M.-Edw. & Haime. The species was originally described from the Maestricht Chalk; and the specimen referred to, which was obtained by Mr. E. Charlesworth, appears to be a *remanié* fossil, derived from the uppermost beds of the Chalk, some traces of which still remain in the neighbourhood of Norwich.

3. “On the Cephalopoda-bed and the Oolite Sands of Dorset and part of Somerset.” By James Buckman, Esq., F.L.S., F.G.S.

In this paper the author discussed the true position of certain beds containing abundant remains of Cephalopoda, found in various parts of the Jurassic region of this country, and of the sandy bed underlying the Inferior Oolite at Cleeve Hill and other places (called by Prof. Phillips the “Midford Sands”), which has been regarded by most authors as belonging to the Lias. From an investigation of the Cephalopoda-bed in quarries at Bradford Abbas in Dorsetshire, the author comes to the conclusion that it is quite distinct from the Cephalopoda-bed of Gloucestershire, and that it is the representative of the Rubbly Oolite at the top of Leckhampton

Hill and Cold Comfort, and of the Gryphite and *Trigonia*-beds of the neighbourhood of Cheltenham. The Gloucestershire Cephalopoda-bed he regards as situated close to the bottom of the Inferior Oolite series; and this is also the position to which he refers the sandy beds above-mentioned.

4. "*Cetarthrosaurus Walkeri* (Seeley), an Ichthyosaurian from the Cambridge Upper Greensand." By H. G. Seeley, Esq., F.L.S., F.G.S.

In this paper the author described a small Ichthyosaurian femur, discovered by Mr. J. F. Walker in the Upper Greensand of Cambridge. He noticed the general characteristics of the femur in Ichthyosaurians, and pointed out, as the chief peculiarities of the bone that he was describing, the subovate form of its head, and the presence of large flattened lateral trochanters, which, if of equal dimensions on both sides of the bone, would have made its greatest transverse measurement greater than its length. Upon this bone he proposed to found a new genus, *Cetarthrosaurus*.

LIX. Intelligence and Miscellaneous Articles.

SOME EXPERIMENTS ON THE EFFECTS OF MAGNETISM ON THE ELECTRIC DISCHARGE THROUGH A RAREFIED GAS WHEN IT TAKES PLACE IN THE PROLONGATION OF THE AXIS OF THE MAGNET. BY MM. AUGUSTE DE LA RIVE AND ÉDOUARD SARASIN*.

IN the first memoir† which we published on the action of magnetism on gases passed through by the electric discharge, we studied first the case of the magnet acting upon a discharge perpendicular to its axis. We recognized that in this case the magnet, between the two poles of which the Geissler tube is placed, has for its effect, besides the deviation of the luminous jet, its condensation, and its more intense brilliancy, a notable diminution of the elastic force of the gas in the portion of the discharge which is more directly submitted to its action. This augmentation of density (produced, at the expense of the rest of the gaseous mass, at the same time as the condensation of the luminous jet) varies with the nature of the gas: it is less strong with hydrogen than with carbonic acid, more feeble with the latter than with air; that is to say, the effect is the more marked the less the gas is a good conductor of electricity. Besides, this effect is more considerable on the portion of the discharge near the negative electrode than upon the rest of the gaseous column passed through by the current. As to the diminution of conductivity of the gas which is known to result in this case from the action of the magnet, we ascertained that it varies also very notably

* This note, written shortly before the death of Auguste de la Rive for the Jubelband of Poggendorff's *Annalen*, contains a résumé of the last experimental researches to which the illustrious savant was able to devote himself whose loss has been so grievously felt by Geneva and the entire scientific world.—EDITOR of the *Archives des Sciences*.

† *Archives des Sciences Physiques et Naturelles*, 1871, tome xli. p. 5; Phil. Mag., Sept. 1871, vol. xlii. p. 211.

with the nature of the gas, being as much more considerable as the gas is a better conductor of electricity.

In the same memoir, we next studied the effect of the magnet upon a discharge which takes place along the line joining its two poles. In this case we observed, on the contrary, a very perceptible augmentation of the intensity of the current. We confined ourselves to establishing that this diminution of resistance, occasioned in the Geissler tube placed axially between the two magnetic poles, is the more marked the better conductor the gas and the less its pressure. With a view to complete our first observations, we have resumed and varied this experiment. May we be permitted to explain here briefly, though they are still far from complete, the results to which our latest researches have conducted.

Instead of arranging our electromagnet in the shape of a horse-shoe as in our former experiments, with the two horizontal bobbins in a line with each other and the two opposite magnetic poles, separated by an interval of 10 centims., which compelled the Geissler tube to be introduced into the cylindrical aperture pierced in the axis of each of the two soft-iron cores, we here employed the electromagnet in a column, so as to cause only one of the magnetic poles to act upon the discharge. The apparatus in which the discharge took place (Geissler tube or a large bell) rested on the upper extremity of the soft-iron cylinder, the line of the electrodes being in the prolongation of the axis of the magnet.

We commenced by operating with cylindrical Geissler tubes of 30 centims. length and 32 millims. breadth, presenting interior electrodes of platinum wire. One of these tubes contains nitrogen, the other hydrogen, both at a very low pressure, about 1 millim. or even less—at least, judging from the appearance affected by the discharge in their interior. The induction-current, furnished by a Ruhmkorff machine of medium size excited by four of Grove's couples, passed through the Geissler tube and then the derivation-apparatus employed in our previous researches. It was on a very small portion of the current, derived into a galvanometer placed far enough from the magnet to be uninfluenced by it, that we observed the variations of intensity of the discharge, according as it was or was not submitted to the action of the magnet, which was excited by 20, 25, 30, and sometimes even 40 Bunsen couples.

Traversed by the discharge from the Ruhmkorff machine, each of the two Geissler tubes exhibits around the negative electrode a beautiful blue aureola extending to the sides of the tube, beyond this a long dark interval, and thence to the positive electrode streaks wide apart. The appearance of this discharge is completely changed as soon as it is submitted to the action of the magnet and when the negative electrode is at the bottom or under the immediate action of the magnetic pole. Indeed, as soon as magnetization commences, the negative aureola, which, with a length of about 35 millims., occupied the whole diameter of the tube, is transformed into a cylinder of only 8 or 9 millims. diameter, very luminous, extending to the positive electrode, across the interval previously occupied by the

dark space and the streaked positive jet, presenting, except the streaks and the colour, an appearance analogous to the narrow positive jet observed at about 8 or 10 millims. pressure.

When, instead of a Geissler tube, we employed a large bell or one of the balloons by aid of which the experiment was made of the auroræ boreales with a central negative electrode encircled by the positive ring, we still obtained the same effect; that is, the large spherical aureola, developed at very low pressures isolated around the negative electrode, was replaced by a narrow blue jet of vivid splendour, having sometimes the appearance of a brilliant blue flame escaping from the positive electrode. This negative jet is always produced in the continuation of the axis of the electromagnet, even when the positive electrode is a ring situated in the same horizontal plane as the negative electrode. The electricity, which escaped equally in all directions from the negative electrode, now issues only in one direction, as if projected to a distance from the magnetic pole. But it is only at very low pressures (1 millim., and even lower) that the effect is produced with this degree of intensity. The greater the elastic force of the gas, the shorter becomes the negative dart, giving place to the positive jet. It is at about 2 millims. that the repulsion apparently exerted by the magnet upon the negative aureola commences to become sensible.

Such is the modification produced by magnetism in the appearance of the electric discharge. It is accompanied by quite as marked an alteration in the resistance opposed by the rarefied gas to the passage of the discharge. As we had already observed, and recorded in the memoir before cited, the magnet has the effect, in the case of an axial discharge, of notably augmenting the intensity of the current.

With the above-described Geissler tube containing hydrogen, placed vertically on the upper extremity of the soft-iron cylinder, the negative electrode beneath the positive, the galvanometer placed in the derived current marked 20° when there was no magnetization, and 40° when the electromagnet was excited by 25 Bunsen couples. The nitrogen-tube, placed in the same conditions, gave 20° without magnetization, and 30° with. In another case, on throwing into the electromagnet the current from 40 Bunsen couples, we saw the deflection of the galvanometer increase, with the hydrogen-tube, from 12° (before magnetization) to 55° , with the nitrogen-tube from 10° to 35° . These examples, taken at random from a great number of analogous results, show that the intensity of the discharge transmitted through the Geissler tube may be quadrupled by the action of an electromagnet sufficiently powerful*. They show, moreover, what we have already recognized, that the effect on hydrogen is more marked than on air, that the augmentation of intensity of the current is greater with the gas which is more conductive than with that which is less conductive of electricity.

* This augmentation of intensity is perceived by simple inspection of the tube, from the fact that the negative electrode becomes red-hot and shows traces of fusion as soon as magnetization commences.

When it is the positive electrode that is submitted to the immediate action of the magnet, there is scarcely any appreciable modification in the appearance and intensity of the discharge. The effect, however, is exactly the same, whatever the direction of the magnetization.

When the circuit contains several consecutive Geissler tubes all placed in the same way upon the upper extremity of the soft iron, each having its negative electrode below, the effect upon the intensity of the current which traverses them all is still greater. But if, in addition to the tube or tubes placed under the action of the magnet, there is one in the circuit out of this action, no effect upon the intensity of its current is produced by the magnet, although the modification which the appearance of the discharge undergoes in the other tubes, placed over the magnetic pole, remains the same. It seems, then, that it is a special and peculiarly intense resistance, having its seat at the issue from the negative electrode, which is thus overcome by the intervention of the magnet.

A final series of experiments support this view, and have shown us that the dimensions of the negative electrode, which notably influence the dimensions of the aureola and the resistance to the passage of the electricity, influence also the augmentation of intensity produced by the magnet in the case of an axial discharge. Working with the large bell, we had a very great, less, or almost no augmentation of intensity, according to whether we employed as negative electrode a platinum point or wire, a small ball, or a ball of 4 centims. diameter.

We confine ourselves here to briefly recording these few observations, without pretending to deduce from them, at least for the present, any theoretical consequence.—*Bibliothèque Universelle, Archives des Sciences Phys. et Nat.* May 15, 1874, vol. 1. pp. 41–48.

EXPERIMENTS ON APPARENT ADHESION. BY M. STEFAN.

By the name of apparent adhesion the author designates the phenomenon that, when two flat plates are laid one upon the other, they cannot be again separated without the expenditure of a force. This phenomenon has been hitherto conceived as conditioned by adhesion—that is, by the molecular forces between the particles of the two plates; and experiments have been made for the purpose of determining statically its amount.

In this phenomenon, however, we have not to do with a static, but with a dynamic problem. The experiments made by the author showed that the separation of the two plates can be effected by any force whatever; only the time in which the distance of the plates is changed a measurable quantity by the action of such a force is the greater the smaller the force.

Simultaneously with the commencement of the action of a separating force the distance of the plates commences also to increase; yet the motion is very slow, and grows ever quicker with increasing distance. The apparent adhesion is much greater when the plates are under water or another liquid instead of in air. The distance

of two plates of 155 millims. diameter, immersed in water, amounting at first to 0.1 of a millimetre, increases, in consequence of the continuous pull of 1 gramme, 0.01 millim. in $1\frac{1}{2}$ minute, 0.1 millim. in 7 minutes. From this it is intelligible how, limiting the observation to a short time, one may be misled to the assumption of a static equilibrium.

The author measured, in his experiments, the times which elapsed while a given initial distance, measured by a wire placed between the plates, increased by a certain quantity. Between these times and the other quantities which varied with the experiments the following relations were found. The times are inversely proportional to the separating force; they are, but not exactly, inversely proportional to the square of the initial distance; for plates of different sizes they are to one another as the fourth powers of the radii of the plates; for different liquids, as the times in which, under equal pressure, equal volumes of the liquid flow through a capillary tube.

From this it evidently results that with this phenomenon the question is a problem of hydrodynamics; and it is now easy, at least in general, to describe it. When the separating force begins to act, the distance between the plates receives an infinitely small increment. Thereby the space limited by the plates is augmented, and the fluid within undergoes a dilatation, in consequence of which the hydrostatic pressure becomes less. The excess of pressure of the exterior fluid acts in opposition to the separating force. Nevertheless equilibrium does not ensue, because the diminution of the hydrostatic pressure between the plates has for its result a flowing-in of the exterior fluid and thereby, again, a diminution of the difference of the pressures. The distance of the plates can be again increased by the separating force, and the same process repeats itself in a continuous manner.

The author gives also an approximative theoretical solution of the problem, starting from the following consideration. The *vis viva* acquired by the plates through the separating force is, on account of the great slowness of the motion, vanishingly small in comparison with the work of that force. This work must consequently have its equivalent in another work; it has it in that which is necessary for maintaining the flow of the fluid from the outside into the space included between the plates.

The equation deduced from this assumption gives again all the different laws to which the experiments have conducted. It permits us also to derive from the experiments the coefficients of internal friction for the experimental fluids. If the centimetre be chosen as the unit of length, the mass of 1 gramme as the unit of mass, and the second as time-unit, it follows that for water of the temperature of 19° C. this coefficient = 0.0108, for air = 0.00183, which values almost exactly coincide with those deduced from the experiments of Poiseuille, Maxwell, and O. E. Meyer.—*Kaiserliche Akademie der Wissenschaften in Wien, Sitzung der mathematisch-naturwissenschaftlichen Classe* vom 30. April 1874.

A SPARK-ADJUSTER FOR THE HOLTZ MACHINE.

BY JAMES J. MINOT.

In the *Annalen der Physik und Chemie*, Bd. cxxxvii. p. 452, and Bd. cxxxix. p. 509, under the title of "Schwache elektrische Funken in Luft, von P. Riess," a method is described of obtaining different kinds of electric sparks from the Holtz machine.

The following method seems to be preferable to that described in the above-mentioned articles:—Having insulated the outer coating of the two Leyden jars which form a part of the ordinary Holtz machine, short thick wires were connected with these outside coatings and terminated in two brass pointers or conductors, so arranged that the distance between them could be varied at pleasure. At first the conductors were placed in connexion with each other; it was then found that a series of sparks were given off between the conductors of the machine. The extreme length of spark obtainable with the machine which was used was 20 centims. The sparks so obtained were large and luminous, passing only at intervals, and requiring a certain electric tension before they would leap across the space. Then the pointers connected with the outer coatings of the Leyden jars were drawn apart about 13 millims.; it was then found that a succession of fine thread-like sparks passed across the space separating the conductors of the machine, whereas there was no such appearance between the pointers connected with the jars. But at intervals a larger spark, not so bright as the normal spark of the machine, would jump across the conductors; and simultaneously with this a similar spark passed between the pointers. This fine line of sparks was found to have a peculiar form, being brightest and largest at the ends of the conductors of the machine, fading away to a lighter and redder tint, and being of a thread-like character in the space between the knobs of the conductors.

It was found, if the distance between the conductors of the machine exceeded a little that between the pointers connected with the Leyden jars, that no large sparks passed between either set of conductors, but only a series of thread-like discharges. When the distance between the conductors of the machine was less than that between the pointers, a similar result was obtained. When the pointers were but a few millimetres apart, a continuous loop-like discharge passed between them, which was not interrupted by the occasional passage of a bright spark, and was not coincident in path with the latter. By varying the distance of the pointers of the Leyden jars, the number and character of the sparks could be changed at will. This method possesses the advantage that by an easy adjustment of these pointers the form of the electric spark can be readily studied. In experimenting upon the passage of the spark through different media, we can by this method diminish the diameter, so to speak, of the spark, and can change quickly from the spark discharge to that of the brush. The change in tone of the sound of the discharge, when the distance between the pointers is varied, is quite marked.—*Silliman's American Journal*, May 1874.

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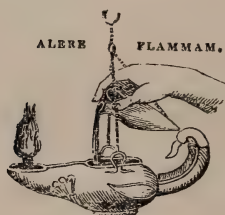
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